Politics and Economics

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Leif Lewin

MAN, SOCIETY, AND THE FAILURE OF POLITICS

ABSTRACT: *Why are political decisions often unfortunate? In replying to this question public-choice theorists fail to distinguish individual conditions from systemic ones. Instead, they make sweeping claims about the egoism of man and the failure of politics. But the real problem is that we often experience government failures despite the best, the most benign motives on the part of citizens, politicians, and bureaucrats. Better than the theory of man's innate self-interest is the theory of the unintended consequences arising from the inherent shortcomings of the political system. To wish well but to do evil—that is the dilemma of politics.*

Public choice constitutes a new theory of politics, write William C. Mitchell and Randy T. Simmons several times in their book, *Beyond Politics: Markets, Welfare, and the Failure of Bureaucracy* (Boulder: Westview Press, 1994), and in his short foreword Gordon Tullock manages to say the same thing—if I have counted right—no less than six times in the space of three pages. But the notion that human beings seek the same personal advantage in politics as in their private lives is one of the perennial themes in the history of political ideas. Machiavelli articulated it, describing as well as any modern public-choice theorist the ruthlessness of the princes. Hobbes was aware of it too, and constructed a whole theory about the results of man's egoistic behavior.

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Leif Lewin, Department of Government, Uppsala University, Skytteanum, Box 514, S-751 20 Uppsala, Sweden, telephone 46-18-18-34-12, telefax 46-18-18-34-09, is the author of *Self-Interest and Public Interest in Western Politics* (Oxford, 1991).

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The ideas of these philosophers were then outstripped by the more "modern" theories of Locke, Rousseau, and Mill, in which men are unselfish and society inherently good. These were then, in turn, replaced by revived theories of egoism, and so on. Thus has the social-science pendulum swung.

But, a modern theoretician might object, public choice is not simply a view of the nature of man. It is also a sophisticated method that demonstrates the logical consequences of the actions of individuals. There is surely something new in that, at least. Since it is systematic, it is superior to the historical accounts of the political science of an earlier age, in which the government was presented as a "romanticized generator of public goods" (Tullock's preface, xiii); the "new" theory more closely resembles economics or perhaps mathematics. Even this claim to "modernity" is unfounded, however. Hobbes, for example, began his career as an historian and wrote on the war between Athens and Sparta. But on one of his trips abroad he visited a library where he happened to see a book on geometry lying open. He read the proposition on the open page and found it impossible to accept. But he also read the proof of the proposition, which led him in turn back to a previous proposition, and so he continued backwards through the whole theoretical construct. When he had finished reading, he was completely convinced of the validity of the original proposition and of the superiority of the deductive method. This was the way all science should be conducted! How amateurish his earlier warnings about civil war now seemed, presented as they were as the products of historiography. He resolved to write about politics in a new fashion—and he did so with considerable pride: he did not consider there to be any scientific works on politics earlier than his own. The similarity of Hobbes's approach to the pioneering public-choice analysis of James Buchanan and Gordon Tullock in *The Calculus of Consent* (1962, 298) is remarkable:

> First, on the basis of certain initial postulates and assumptions, the logical consequences can be developed. This sort of theorizing is purely logical in nature and has no empirical relevance in the direct sense.

Perhaps it is hypercritical to take exception to the intellectual pretensions suggested by a certain tone? But in speaking about "a revolution in thought," "scientific advances," and "out-of-date opinions," Mitchell and Simmons make use of bad history to get their

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readers to be unfavorably predisposed toward earlier writers; thus, they rather persuade than convince their readers. The time at which something is said has no bearing on its validity. Instead, let us judge each theory on its substantive contributions and declare that what Mitchell and Simmons have done is to remind us of *one* of the classic theories of the motives of man and the nature of politics that recur throughout the ages. They do so, I hasten to add, in an unusually thorough and stringent manner that clarifies their subject. Nevertheless, the fundamental question is whether they and their forerunners are right.

Self-Interest

If, like the authors, one has set oneself the task of writing a basic textbook in public-choice theory, it is certainly to the point to begin with the assumption that the self-interest of man is the moving force in politics. The electorate is presented as a group of pocketbook voters (instead of motivated by a desire to see some general interest realized), the politicians as vote maximizers (as opposed to program-realizers), and the bureaucrats as budget maximizers (instead of as loyal implementers of public decisions). "The new approach begins by assuming that voters are much like customers and that politicians are much like businesspeople," Tullock writes (xiii). "Bureaucrats in the federal government are much like bureaucrats in many large corporations. These statements seem simple and straightforward. One would think few would disagree."

Why? An equally plausible hypothesis is that politics has emerged in *protest* against the market. Parties are often formed because various groups of citizens are moved by a passion to *change* the conditions they see around them in society. "When we go to vote, we put on our Sunday best," members of European populist parties used to say. Through an active interest in politics one could broaden one's horizons and take responsibility for things other than those related to oneself and one's family. This is why John Stuart Mill saw democracy as an education of the public spirit, a doctrine that was on everyone's lips when the general franchise was introduced at the time of the First World War.

Two equally reasonable a priori assumptions thus stand here opposed to each other. The task of the scientist is to confront these ide-

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ologies with empirical data. In this respect, modern political science has much to contribute; here, one really can speak of a definite advance in relation to former generations of scholars. That Mitchell and Simmons have not made a closer study of the very extensive empirical research that has by now been carried out into the motives of politicians, bureaucrats, and, in particular, voters is a weakness. This negligence is typical of public-choice theory, with its logical and deductive orientation. But similar faults can be found in other approaches in political science. A Tower of Babel has risen amongst us, for those who conduct empirical electoral studies or do empirical research on politicians and bureaucrats often ignore the theory of rational choice, which they might need to interpret their data. Not to mention those who study the history of political thought, who often forget that the philosophers they so lovingly interpret, long since dead and buried, are in fact very much alive, although in the guise of modern theorists (Buchanan is actually Hobbes, Arrow is Condorcet, Hayek is Hume, Nozick is Locke—the game could be continued); and these scholars are just as guilty of neglecting modern empirical research, which would make it possible for them to test the validity of a goodly number of the statements about man and society that have been made by the philosophers they study.

The first empirical results of electoral research tended to support the self-interest hypothesis. Researchers were able to determine a correlation between (of all things) weather and support for the incumbent president: if the harvest was good and the economy prosperous, the president got re-elected (Barnhart 1925). Man thus appeared to be an egoistic creature interested only in satisfying his material needs. However, the next question was *which* of the voters deserted the government when times were bad, e.g., during a period of mass unemployment. If the deserters were those who were affected, this fact could be taken as support for the self-interest hypothesis. If, on the other hand, they were voters who, without themselves suffering from the crisis, sympathized with those affected and based their votes on principled convictions about what sort of policy the country needed to counteract unemployment, this result would rather favor the counter-hypothesis; in this case, the public interest would seem to be the motive force (Schlozman and Verba 1979).

Present-day research, with its refined concepts and methods, presents us with the following picture. Economic considerations are indeed decisive for the voter's choice of party. But it is the state of the

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country's economy as a whole rather than the voter's own pocketbook that is appraised: what tips the balance is the voter's assessment of the government's ability to manage unemployment, inflation, and the budget deficit. The extra that they stand to gain if a certain party takes power means less to most voters than the loss in welfare that would befall all if an incompetent or ideologically repulsive government were to come to power. There are pocketbook voters in every country, but surprisingly few of them (Kramer 1971; Kinder and Kiewiet 1979; Fiorina 1981; Kiewiet 1983; Lewis-Beck 1986). The theory of voter self-interest has been described as "a good hypothesis facing resistant data" (Lewis-Beck and Eulau 1985, 4).

The deficiencies of Mitchell and Simmons's treatment of empirical data become apparent on page 46, where they dismiss the ideal of active and interested voters by pointing to the low level of participation in elections in the United States. The reader might come to the conclusion, probably unintended by the authors, that European voters, with 90 percent participation in elections, are consequently very "interested." (In actual fact, more relevant data indicate that the level of participation is rising generally in the Western world and that American voters actually tend to come closer to the classical ideal of a democratic citizen than their European counterparts.)

Similarly weak is the evidence supporting the public-choice hypothesis regarding politicians. Mitchell and Simmons describe with irony the naïveté of Keynesian economists who believed that governments would use their post-1920s economic powers for the purpose of improving the welfare of their citizens. Instead, they maintain, governments naturally use their increased power for the selfish purpose of getting re-elected, creating a political business cycle that distributes benefits (such as Social Security payments) to voters in election years but diminishes them in the period between elections. Is this true? It is a matter of some controversy among researchers, but a consensus has gradually formed around the verdict that the political business cycle is unproven. (Sweden in 1979 is often cited as a curious exception; the minority Liberal government, comprising ministers who were trained in the social sciences, expanded the budget just before a general election.) The voters are not so easily fooled; if benefits are withdrawn, they will certainly notice it and remember at the next election (McCallum 1977; Alt 1980; Paldam 1981; Dinkel 1981).

Finally, it also has proven difficult to verify the hypothesis that bureaucrats are budget-maximizers who try to get large budgets so that

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they, themselves, can more easily obtain a higher salary, larger office, or more luxurious car (Niskanen 1971)—as appealing as that view might be to our prejudices about bureaucrats, with whom most of us have had at least some unhappy experience. Instead of administering an agency with a large budget, "the Mandarins of Western Europe," for example, dream of running a small efficient unit close to the ear of the prince—that is, the finance minister. The satisfaction of being close to political power and of being allowed to take part in executing its program is still today more appealing to the top bureaucrats than the task of administering large, expensive systems for making routine payments (Goodin 1982; Breton and Wintrobe 1975; Rose 1984; Dogan 1975).

The Common Good

If it really were true that man is essentially an egoist in politics, surely a Leviathan would then be needed to prevent people from causing each other injury. That was the conclusion Hobbes drew, as is well known. And he presented Leviathan as nothing less than a fire-breathing crocodile that suppressed people—not at all as harmless as the authors seem to think when, on page 5, they write of "minimal activities by government" as the normative implication of public-choice theory. They do mention Hobbes in one place, but unfortunately do not get his position quite right; a keen awareness of the history of political thought is not one of the authors' strong points. Hobbes delighted in pointing out conflicts between goals. To decide between freedom and security was no easy choice. For the security he thought necessary, Hobbes was prepared to accept a form of political absolutism that would horrify any modern public-choice theorist.

To me, this is a point on which public-choice theorists have always seemed to be illogical. If man is inherently "good," no state is needed to maintain law and order, but it is very definitely required if man is inherently "evil," as is assumed in their theory.

The authors' normative position is summarized in many pithy formulations. Their main point is a criticism of the notion that market failures justify government intervention. Government is presented as something quite different from the benevolent state that political scientists and welfare theoreticians often claim it is. On the contrary,

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Table 1: Conceptions of Politics

		Politics	
		"Bad"	"Good"
Individual	"Bad"	Hobbes	Smith
	"Good"	Epicurus	Rousseau

it is a tool in the hands of those who wish to gain benefits at the expense of others and as such, government should be made as small as possible.

Adam Smith provides the authors a solution to the conflict between egoistic individuals and a mere minimum of state coercion: when individuals act in a free market solely with their own interests in mind, they are also led, as if by an invisible hand, to further a general goal (such as economic growth) that was no part of their original intention. The authors thus declare their faith in the market economy and take a number of pointed and humorous shots at their opponents; in a section on "agoraphobia" in chapter 8, the ignorance shown by many of the critics of the market economy is particularly telling.

But suppose the market is characterized not only by an invisible hand but sometimes by the "Prisoner's Dilemma"? What happens if selfish individual actions lead to suboptimal outcomes? This is the misgiving the authors should feel when they concern themselves with the free-rider problem (Olson 1982).

Perhaps this is the point at which I might try to summarize the possible combinations of individual strategies and collective results that suggest themselves during a reading of Mitchell and Simmons's profound analysis. Public-choice theory is so rich and varied that it can be interpreted from a number of perspectives. Let us arrange the combinations in terms of classical positions held in the history of political thought.

For Hobbes, mankind was "bad"; men were like wolves toward each other and were moved by instincts and drives to exercise power. Leviathan kept them in order. But there was never any doubt that this suppression was also a "bad" condition. The problem was simply that the alternative is even worse: the civil war that would unavoidably

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break out if men were not kept in check by the iron hand of the state. It has become common in the public-choice literature to agree, a bit nonchalantly, with Hobbes's questionable analysis; in this respect Mitchell and Simmons are no exception.

Smith, on the other hand, does not believe any such state tutelage to be necessary. In his view, the selfish preferences of the individual are transformed by the invisible hand into the general interest. Smith and other early liberals thought that what is best for the individual is best for society.

For Epicurus man was good; this was his "happy message" when trying to combat the predominant Platonic outlook of ancient Greece, with its depreciation of the earthly life. Politics, on the other hand, was "dirty and bad"; it was best to keep away from it. The purpose of life was to gather one's friends together in the garden for philosophical discussion. In a way, this is the dream of public-choice theorists, perhaps of everyone. But does the minimal-state utopia work in the present-day world?

Rousseau combines the best of all worlds: man is good and society is good. Despite this, he is no naive idyllist. On the contrary, he is the one who expressed the modern predicament in the sharpest terms: how are the individual preferences of citizens to be transformed into optimal collective decisions? What happens when individuals become active in politics in order to seek general solutions to the problems of society? How can the "general will" be attained? Politics is a "black box" into which individual preferences are stuffed and out of which unfortunate decisions sometimes emerge. What takes place inside this box? As I see it, Rousseau's thoughts constitute the most fruitful point of departure for a study of the problem posed by the public-choice school. Let us therefore continue the discussion on the basis of his view of the problem as I have expressed it.[1]

But first I should like to put into question the very point of departure of our—and the public-choice school's—analysis. Is it really necessary to make any a priori assumptions whatsoever about the egoism of man—whether the nature of man is "bad" or "good"? These a priori assumptions have done more harm than good. In the social sciences it is wise to follow the principle of minimal definition: to refrain from presupposing more than is necessary to the research task at hand and to leave instead as much as possible open to empirical testing (Sartori 1984). I should therefore like to propose that we do not make any assumptions at all about whether man is "bad" or

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"good"; such speculation is unnecessary if we wish to explain how political decisions are made. Our task is instead to open the black box of politics and examine what happens with the preferences of individuals when they become involved in collective decision-making.

The Black Box of Politics

Political decisions often are unfortunate. They may have sprung from good intentions but the outcome is often bad. Why is this so?

The public-choice approach as presented by Mitchell and Simmons has valuable contributions to make in tackling this question. Democracy was conceived of as a way of substituting the power of the people for the absolutism of kings. But what happens if the people are not in general agreement? This was a theoretical oversight that, during the French Revolution, was shown to have significant practical consequences in reality. The majority principle was finally adopted to break the deadlock. But this rule is of little use when majorities can so seldom be found in reality. In Western democracies, it is in practice different minorities that rule by means of cooperation, negotiation, and bartering. In the process they must sacrifice some of their preferences, which they have been charged by their voters to defend, as payment for receiving help on other issues (Buchanan and Tullock 1962). Sometimes it is not possible to arrive at stable decisions because preferences are circular and do not meet the requirement of transitivity (Arrow 1963). On occasion, the spontaneous wish of the people is too hasty; without special constitutional arrangements, democracy suffers from a lack of reflection and examination of consequences (Riker 1982). Given these difficulties, real-world political decision making easily takes on the character of rule by an isolated elite, something that has little in common with the original theory of democracy. It is not surprising, therefore, to find that the public-choice school often expresses contempt for politicians.

Perhaps most important of all, when trying to shine some light into the black box of politics, is to maintain the "methodological individualism" for which Mitchell and Simmons energetically plead. "The state" is a mere intellectual construct. All that we can observe are individuals and their actions. For this reason, it is an error in method to suppose with some political scientists or welfare theorists that "the *state* is benevolent." Individuals, alone, can have such charac-

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teristics (Weldon 1947). As I have pointed out above, however, it seems to me that public-choice theorists turn a logical somersault when they presuppose that all people can have only one kind of character—namely, always "egoistic" and never "benevolent."

The Long Run

How can we escape from the predicament in which unfortunate decisions are made despite the often benign intentions of those who made them? Game theorists have shown that through repeated rounds of the "Prisoner's Dilemma" the players learn to cooperate in order to arrive at collectively rational solutions. Repetition is one of the hallmarks of politics; tomorrow we meet again for a new round of negotiations. Credibility is therefore a politician's most valuable asset; if one is deceitful today, one will be left out of the bargaining tomorrow (Axelrod 1984). Rousseau made the same point in the famous parable of the stag hunt, even though he wrote more intuitively and without the stringency one finds in modern rational-choice theory: before man "discovered tomorrow" and learned to make long-term calculations, it could happen that a hunter, patiently standing watch in the stag hunt, might allow himself to be distracted by a rabbit that happened to run by, even though his share of the stag would have been greater than the rabbit he might have for himself (Rousseau [1755] 1974).

Mitchell and Simmons, as well as the whole public-choice school, depict political man as such a myopic cave man. But is not politics the art of making farsighted assessments of the consequences of collective action for society as a whole? Is this not, in a nutshell, the purpose of an election campaign? The authors in fact admit as much in their discussion of the political business cycle. "The most important cause of modern economic fluctuations is not the economy but the polity," they first state (176). But suddenly, they argue instead that the voters are not so foolish as to let themselves be deceived time and time again: "the citizenry soon learn that they can partially counter this obvious Machiavellian electoral strategy" (180). Thus, while those who control the state, i.e., politicians and bureaucrats, are constantly out to hoodwink us, this is perhaps not so serious since the citizens can fight back! As far as I can see, this constitutes an important concession: man is *not* as shortsighted as public-choice the-

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ory assumes but is capable—not from goodness but from rationality—of making farsighted calculations.

That being the case, the theory of public choice, as presented by Mitchell and Simmons, contains a sort of ecological fallacy. Their assumptions in one area lead them to believe that similar conditions pertain in another. Let us instead be content to declare that politics sometimes fails, but let us not make any a priori assumptions about how frequently. Let us assume that man is capable of acting rationally without presuming that he always does. And certainly let us engage in no apriorism about the substance of man's preferences. The better we understand what happens in the black box of politics, the better able we surely will be to make long-term calculations. Political scientists have only just begun this project by examining the institutional conditions that sometimes lead to unwanted social consequences.

NOTES

1. J. S. Mill offered a special variant, a sort of mixture of Smith and Rousseau. This eclecticism makes him difficult to classify. Basically he has an optimistic view of the nature of man (man is "good") but he also counts on there being egoistic beings. Mill's original contribution to democratic theory is to see the role of democracy as that of educating citizens: by being trusted with public positions, all people—the "bad" as well as the "good"—are transformed into responsible citizens.

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· *Anthony Woodlief*

UNFORESEEN CONSEQUENCES AND PATHOLOGICAL SELF-REINFORCEMENT: WHY CITIES DECLINE

ABSTRACT: *Peter Salins's* The Ecology of Housing Destruction *and Salins's co-authored work with Gerald Mildner,* Scarcity by Design, *provide fascinating evidence of the unintentionally harmful effects of urban policies, and their role as catalysts for further harmful policies designed to ameliorate previous harms. The result is a web of counterproductive regulations confronting bewildered policymakers and frustrated citizens. In his* The Federal Government and Urban Housing, *on the other hand, R. Allen Hays not only misses the existence of these complex policy dynamics, but fails to assess accurately the consequences of the housing policies he favors. His work does, however, offer useful insights into the nature of policy decisions.*

While many urban theorists maintain a rather sanguine view of government housing policies, Peter Salins and Gerald Mildner argue, in *Scarcity by Design* (Cambridge, Mass.: Harvard University Press, 1992), that the impact of such policies has often been literally devastating. In an earlier work, Salins (1980) documented how a combination of rent regulations, stringent zoning codes, and short-sighted low-income housing policies have produced slow-spreading "zones of destruction," littered with abandoned buildings, rubble,

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Anthony Woodlief, e-mail woodliet@kochind.com, is completing a doctorate in political science at the University of Michigan.

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and hopeless, poverty-stricken adults and children. In their collaborative endeavor, Salins and Mildner bolster and update Salins's earlier analysis. The work of these authors illustrates how policy makers often harm the people they intend to help. I will argue that this is not only a common outcome, but a result that is difficult to change even when the policy makers themselves recognize the harm caused by a policy after it is implemented. What is more, we have reason to expect such outcomes even if lawmakers are rational and altruistic.

Fighting Human Nature

Unlike many social scientists, Salins and Mildner are attentive to the effects of public policies on behavior. Consider rent control, a primary culprit in Salins and Mildner's analysis. Though New York City has several layers of rent regulations that fall under the rubric of what outsiders call "rent control," to some degree they all have the same effect: they keep the rents of regulated apartments below what a free market between tenants and landlords would produce. Like any other price control, rent regulation affects the decisions of individual buyers and sellers. Salins and Mildner provide data to show that such effects include a restricted supply of new housing, the decay of existing housing, an inefficient distribution of housing, an increase in property-tax delinquency and thus in building abandonment, and a decline in city property tax revenues.

While some of these effects are easily anticipated, others are less so; thus, it is worth briefly considering each. By imposing an artificially low price on housing, rent control reduces the revenue generated by rental properties, in many cases below their costs. Once their earnings dip below maintenance costs and taxes, owners have no incentive to make repairs or pay taxes, and every incentive to abandon their buildings. Rising rates of delinquency and abandonment translate into falling property-tax revenue. Meanwhile, the city housing supply contracts, as existing buildings deteriorate and developers concentrate their resources elsewhere, for fear that any new apartments they construct will be regulated like existing apartments. Finally, rent control produces housing inefficiencies by creating a strong incentive for people to remain in their regulated, low-rent apartments, even if their housing needs change—say, because their

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children move out. Thus we see young families with children crammed into small apartments, while across the street live elderly couples with three bedrooms.

Salins and Mildner show that the benefits of below-market rents appear to accrue primarily to upper- and middle-income families. While the rents of regulated apartments are on a par with those of unregulated apartments in most of New York City, a substantial gap exists between the two in Manhattan. Average incomes were 75 percent higher in this borough than the rest of the city in 1987. According to a New York City developer, "'We've got plenty of low-income housing in New York. We've just got upper-income people living in it'" (13).

But rent control is not the only cause of New York's zones of destruction. Salins had argued that another key event in New York City's long march to its present sad state was the marriage of rent delinquency and tenant grievance proceedings under the auspices of the New York Housing Court in 1972. Because tenant complaints took precedence over rent delinquency complaints, rents became much more difficult to collect in poorer, crime-ridden buildings, where more maintenance is necessary and where tenants are more likely to break things in order to create a maintenance grievance with which they can avoid paying rent.

This situation was exacerbated by a New York City Department of Social Services (DSS) policy of providing its beneficiaries rent and moving allowances with few restrictions. Landlord-tenant adjudication procedures, which were slow and subject to abuse by unscrupulous tenants, allowed DSS subsidy recipients to withhold rent for several months, pocketing the DSS subsidy. When evicted, the tenants could relocate with the help of a DSS moving allowance. To convey the magnitude of the delinquency problem, Salins noted a late 1970s city estimate that rent delinquency among the welfare population was 36 percent (1980, 46). While this behavior led many landlords to refuse DSS recipients as tenants, Salins argued that profit-maximizing owners found that they could make short-run profits in spite of high delinquency rates by accepting DSS clients, doing little maintenance, and not paying property taxes. When delinquency, rent withholding due to poor maintenance, and waning tax grace periods made their buildings unprofitable, these owners simply abandoned them.

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"Zones of Destruction"

Owners might have been expected to prefer long-run earnings to the short-run strategy described by Salins. But remember that the tenant composition of each building affects the entire neighborhood, and hence the value of other buildings. Crime is highly positively correlated with factors, such as low income, that were themselves positively correlated with the likelihood of receiving a DSS housing subsidy. If other owners began to accept a significant number of DSS subsidy recipients as tenants, owners who did not do so would still have seen their incomes fall as middle-income tenants fled the neighborhood, a cost that would have risen as the fears of increased crime proved justified.

Similarly, rent control and other regulations, by reducing the profitability of building ownership, may create the conditions under which all owners have an incentive to let their buildings decay—even owners who are not directly affected by the regulations. Though Salins doesn't say so, the building owners in a neighborhood face a multiple-person Prisoner's Dilemma (Smith 1982; Axelrod 1984; Olson 1965). Each will be better off if all refrain from letting their buildings slip into disrepair or renting to unruly DSS tenants, but each will suffer if he refrains while others in the neighborhood defect and pursue the short-run strategy. An owner who decides to pursue the short-run, low-maintenance strategy increases the chances that the other owners in the area will likewise defect. Each must worry that enough defections by his fellow owners will create for him a real loss, as opposed to the loss comprised of the opportunity cost of pursuing the short-run rather than the long-run strategy.

The results of this environment are Salins's "zones of destruction," where the short-run strategy has resulted in large-scale abandonment. Housing on the edge of these zones, in turn, is "programmed for abandonment" as owners do the equivalent of selling in a declining market (Salins 1980, 93). This process allows the zones to spread slowly, leading economists to describe New York City rent control (to which Salins adds other policies) as slow-motion bombing. To substantiate his claims about building abandonment and spreading zones of destruction, Salins examined the acquisition costs, operating expenses, and rental income of several properties either adjacent to or far from zones of destruction. He found that even though rental in-

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come and operating expenses were roughly the same for all of the properties, the acquisition costs were much higher for the properties located far from zones of destruction than for properties adjacent to the zones, reflecting awareness that the properties near zones of destruction have much lower long-run income potential (1980, 98).

Though the adoption of a short-run strategy by some owners may well lead others to do the same, we might still ask what prompts the first defections in a neighborhood. Salins seems to attribute the owner's choice of a short-run strategy solely to the decline in potential long-run income resulting from being close to a zone of destruction. This decline reduces the difference between long-run profits and short-run profits, making the former a less attractive alternative. But this explanation begs the question by assuming the pre-existence of the very decay Salins is trying to explain. Costly rent, zoning, and building codes may be decisive here, by reducing the long-run income to be expected from owning a building. Add to this the fact that an owner's assessment of his potential long- and short-run income is a function, in part, of his expectations regarding the other owners' actions, and we see how instability can develop in a neighborhood. If for any reason owners lose faith in the willingness of others to stand firm in the face of encroaching decay or declining earnings, the defections begin to accelerate, fueling a downward spiral.

Policy Self-Reinforcement and Autocatalysis

Salins's work draws our attention to an important but overlooked component of public policies—their capacity to perpetuate themselves, reinforce one another, and serve as catalysts for chains of unforeseeable events that, once set in motion, cannot be undone by removing the policy that sparked them. Most scholars tend to assess the effects of public policies on income, housing, paperwork requirements, and the like. Others, including Salins and Mildner, focus on the behavioral effects of policies—how they modify and sometimes pervert human action. But too few thinkers explicitly consider the effects of policy on policy. As Aaron Wildavsky (1987, ch. 3) observed, policy very often becomes its own cause, as we endeavor to correct the unintended consequences of previous decisions.

Government programs often fail to resemble the rational, comprehensive planning that serves as a model for aspiring policy makers.

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Limited time, limited information, multiple lines of authority, and multiple issues competing for their attention leave officials ill prepared to craft well-considered legislation, let alone perform the monitoring necessary to ensure faithful implementation. Public officials have a basic inability to predict the multitudinous effects arising from the implementation and interaction of their policies.

An understanding of this phenomenon is essential because inevitably the attention of any student of politics and government (perhaps especially urban politics and government) turns to a deceptively simple question: "Why?" Why do cities erect cumbersome, unresponsive bureaucracies and tolerate their resulting malfeasance? Why is a policy subsystem like New York's housing apparatus so difficult to change when most citizens are displeased with its performance? Why can't public officials make better policies? These questions raise doubts about the efficacy—indeed, the very purpose—of government.

While political scientists, historians, and economists offer several powerful models that might be used to answer these questions, they prove ultimately unsatisfactory. Pluralist theory, for example, suggests that governments make policy in response to interest-group pressures. Indeed, Salins and Mildner reveal how landlords' associations, tenants' rights groups, and developers have at times influenced the adoption or rejection of various policies in New York. But even in a strictly pluralist world, where the preferences of government officials have no effect, this model would be inadequate to explain New York's housing mess. That is because it does not take into account ignorance—on the part of interest groups and public officials alike—regarding the present and future consequences of policies. If we realize the extent to which policy making involves reaction to these unintended consequences and acknowledge how quickly this can produce layers of conflicting law and bureaucracy, the notion that policy is strictly the result of interest-group demands loses its force. In addition, we know that many interest groups concerned with particular policies spring into existence only after the policy is enacted, which turns the pluralist argument on its head. Finally, a great deal of interest group pressure, while powerful, is blunt—it offers no guidance to policymakers, only a clear expression of dissatisfaction with the status quo.

A focus on policy makers, on the other hand, produces various forms of public choice theory (Croswell 1975; Niskanen 1971; Tullock 1965) and state theory (Krasner 1984), according to which gov-

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ernment officials pursue either self-interest or their judgments of what is best for the public. Budget and regulatory growth can be explained quite nicely, it would seem, as functions of elite preferences. Political elites are themselves divided on policy issues, however, forcing them to seek compromises that satisfy neither ideological faction. These compromises, joined with the boundedly rational nature of decision making (Cyert and March 1963; Lindblom 1959; Simon 1957), help produce policies that generate unanticipated consequences. These unintended policy consequences quickly turn even the most rational, self-interested policy maker into a harried troubleshooter, not a foresighted planner.

Incrementalists (Braybrooke and Lindblom 1963; Lindblom 1959 and 1965; Wildavsky 1988, ch. 3) incorporate the problem-solving, crisis-motivated nature of policy making into their models to yield a fairly realistic picture of government bureaucracies. They argue that in the face of limited time and information, confronted with an environment of competing interest groups and ideological or self-interested bureaucrats, policy makers adopt a rational strategy of gradual change based on feedback from past changes. Such an approach enables policy makers to deal with the impossibility of measuring tradeoffs between, say, roads and hospitals by making small adjustments in the budgets for each and then assessing whether these adjustments produce desirable outcomes (Lindblom 1959). It also keeps them from making large mistakes: if last year's actions weren't a colossal failure, then actions slightly different from last year's shouldn't be disastrous either. The first rule in politics, then, is to avoid blunders for which one can be held publicly accountable. This, according to the incrementalists, explains why budgetary changes are usually small from year to year, and why public policy is so often characterized by an evolutionary process of changes in rules.

The beauty of the incrementalist model is that it seems not only to describe much public policy, but to imply the possibility of arriving at optimal policies in the long run. If policy makers are groping about in the dark as a result of limited information about the future consequences of their actions, then a process of gradual adjustment and feedback can produce the best policy. This attractive implication, however, belies the model's ability to explain policy. While it is inarguable that policymakers often adopt an incrementalist approach for precisely the reasons suggested by these theorists (limited information, limited time, inability to compare apples and oranges, etc.), it is

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also quite clear that policy—in our case housing policy—very often is *not* optimal.

Furthermore, policy arenas are occasionally characterized by sharp changes that incrementalist theory neither predicts nor explains (Schulman 1975). Bryan D. Jones (1994) and Frank R. Baumgartner and Jones (1993) argue that the inherent inertia of policy-making organizations, combined with the limited attention range of voters, produces punctuated policy equilibria. While policy inertia and incrementalism are the norms, once some event (either internally generated or exogenous) refocuses public attention on a policy and its problems, the stage is set for drastic change as policy makers respond. The general tendency toward inertia and self-reinforcement then reasserts itself, as interest groups and elites coalesce around incremental modifications of the new policy.

Acknowledging the ability of policy to reinforce and perpetuate itself takes us a further step toward reconciling incrementalist theory (as well as the other aforementioned models) with observed reality. When we consider the additional constraints of limited information and time, shared authority over decisions, and competing private and bureaucratic interests, we can see how actual policy making begins to diverge from the predictions of pluralists, state theorists, public-choice theorists, and incrementalists.

Admittedly, there is no great revelation in the claim that public officials enact policy without precise knowledge of how closely it mirrors the "ideal." What the foregoing theories have failed to capture, however, is that as this gap between implemented policy and ideal policy becomes apparent, officials find themselves constrained from acting on this new knowledge. The policy they have created causes problems they did not foresee, which they may endeavor to repair with—of course—more policies (which carry their own unintended consequences). But the implemented policy finds proponents in both private and bureaucratic actors, who defend its perceived benefits for their interests and ideals. Those harmed by the policy, on the other hand, are often dispersed and unaware of the damage it causes. Soon, policy makers find themselves scrambling to patch up problems created by the actions of their predecessors, all the while attending to the varied demands of interests engendered and fueled by these earlier policies. Over time, they travel down a path that nobody intended or foresaw, yet which nobody seems capable of leaving.[1]

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The Tangled Web

Though this is not their primary intention, Salins and Mildner provide many examples of self-reinforcing, self-perpetuating policy streams that have produced an unintended maze of constraints on beneficial human action in New York City. For example, the city's numerous extensions of rent control statutes, interspersed with promises to refrain from doing so again, served to create uncertainty among potential developers about the future regulatory treatment of any apartment buildings they might erect (59-68). This uncertainty hampered growth in the supply of housing, causing higher rents as supply lagged behind increases in demand. Rising rents elicited popular demands for the extension of rent control to currently uncontrolled housing. This only made developers even more wary of building apartments.

The most striking example of a regulation's capacity to reinforce itself can be found in the method New York city and state officials periodically use to assess the necessity of continuing rent control. State law requires a vacancy survey every three years to verify the persistence of a housing emergency, defined as a vacancy rate under 5 percent (41). Insofar as rent controls *create* a housing shortage, this procedure virtually guarantees that they are self-perpetuating.

New York City's housing policies have served not only to reinforce themselves; they have perpetuated a stream of additional policies that creates an even more complex regulatory labyrinth. For example, Salins and Mildner show that housing construction in the city increased in anticipation of stricter zoning implementation in 1961. Before this law, housing in New York could be built on land zoned for manufacturing or industry. The new law promised to reduce the supply of available sites significantly by requiring housing to be built only on lots zoned as residential. Naturally, builders raced to beat this new law, generating a housing boom that was followed by a drought after the new regulations took effect. The resulting near-cessation of construction caused a jump in the rents of unregulated apartments, producing new demands for an extension of rent control, which officials eventually granted.

As the city's economic base shifted from manufacturing to service industries, the buildings associated with the former fell into disuse (72). New manufacturers opted for the suburbs, where there were lots that could accommodate long, low buildings. But the 1961 law prevented

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turning the abandoned manufacturing buildings into apartments, despite chronic housing shortages. Predictably, the owners of the abandoned buildings began illegally renting them to residential tenants. At this point the authors' account takes a comic turn, as the city, which refused to legalize this practice entirely, created a Loft Board that decided that only "artists-in-residence" could live in these buildings. Applicants were required to submit samples of their work to the city's Department of Cultural Affairs to obtain an artist's permit, which they could then take to the Loft Board to obtain permission to live in a loft (73). Fifteen years after passing the zoning law, New York officials were still passing laws to cope with its unintended consequences, and today they are legislating remedies to the consequences of *those* laws.

In fact, New York has erected entire bureaucracies to deal with the consequences of its regulation. The remedy these agencies provide is more regulation. Salins and Mildner note that any reasonably large apartment-building proposal has to go through a variety of hearings where it can be blocked by a wide array of local interest groups. Meanwhile, the regulation-driven process of decay and abandonment Salins described has created what he and Mildner label "public housing by default" (92), as the city steps in with its own buildings to remedy the housing shortage its policies helped create.

As one begins to uncover the layers of policy governing housing in New York, a set of malign symbiotic relationships between regulations, agencies, landlords, and tenants becomes apparent. Regulations intended to make housing affordable create incentives that restrict the housing supply, which creates disputes among and between sellers and buyers. Tenants murder landlords. Landlords demand exorbitant "key money" from prospective renters. Squatters occupy abandoned buildings. Regulations and agencies created to handle these disputes (e.g., the New York City Housing Court) promulgate more rules and become ensnared in contentious relationships both with existing agencies and with citizens. Once set in place, this web thickens and evolves to the point that nobody can understand or predict its behavior, everybody is unhappy with it, yet no one seems capable of fixing it.

Stepping Back in Order to Move Forward

In addition to perpetuating themselves and catalyzing additional government action, policies may reach out to affect social norms. Norms

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are themselves examples of self-reinforcing systems. Insofar as public policies affect behavior, they may have the ability to alter norms in unintended ways, replacing norms beneficial to individuals and groups with harmful ones. The growth of urban pathologies such as crime, drug use, and illegitimacy may be explained in this way. To see how this is so, it is helpful to consider the work of someone who would disagree with such an analysis, and who seems unaware even of the policy tendencies described by Salins and Mildner, let alone the self-reinforcing nature of policy that I am suggesting.

R. Allen Hays's *The Federal Government and Urban Housing* (Albany: State University of New York Press, 1995), though at times myopic, is by far one of the better treatments of federal housing policy. Hays applies a model of elite dissensus (of which, more later) to explain the shortcomings of such federal policies as public housing, low-income housing subsidies, rent vouchers, housing rehabilitation, and community development. In Hays's view, these programs suffered from compromises between liberal and conservative elites that left them underfunded, poorly targeted, and too short-lived or underanalyzed for us to appreciate their effectiveness. Hays also marshals demographic and economic data to argue for a renewed federal commitment to housing programs, in the face of what he believes to be the growing gap between those who can and cannot afford decent homes. Unfortunately, much of his data is misinterpreted or incorrect, while his conclusions require a suspension of interest in fundamental questions concerning poverty. Speaking more generally, the relative merits of this book say more about the doldrums of contemporary urban studies than the success of Hays's approach. Despite these shortcomings, Hays offers ample evidence with which to support and extend the hypothesis I have constructed so far—that the policy realm yields unintended effects that are self-perpetuating and autocatalytic.

For some time, poverty researchers have been split between those who believe that external forces create poverty, and those who fault the pathological behaviors of the poor for their condition. Though it is central to any analysis of the successes and failures of housing policy, Hays appears unconcerned with this debate. Thus, he is able both to claim that public housing projects failed because they accepted too *many* of the very poor (who, Hays doesn't say, tore the places apart), and to advocate the construction of more such monstrosities for the very same poor. The same goes for low-income housing subsidies, where the admission of very poor people raised the default rates. A

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recurring element in Hays's critique of major federal housing programs is that their availability to the poorest of the poor gave them a scent of failure that tainted their many successes in giving people better housing than they might otherwise have obtained.

For example, Hays believes that negative public impressions of housing projects were fueled in part by the gradual admission of AFDC recipients (Aid to Families with Dependent Children, given largely to jobless single mothers). According to Hays, "This helped reinforce public perceptions of the program as one more 'dole' to those already receiving aid" (95). Renting to this very poor group, in other words, made public housing look bad, which affected its ability to secure future funding from public opinion-conscious politicians. The truth, of course, is that it *was* one more dole to those already receiving aid. Furthermore, since most Americans don't peruse the tenant rolls of housing projects, or know anyone who lives in them, the negative impressions Hays mentions are likely to have stemmed not from public misperceptions of the moral worth of their residents, but from seeing what these projects looked like and hearing daily news accounts of very real and often brutal crimes done by and to their residents.

Like most academics, Hays seems to accept the thesis inspired by Richard Cloward and L. E. Ohlin (1960) and Frances Fox Piven and Cloward (1971) that poverty causes crime (and, by extension, drug use, suicide, divorce, illegitimacy, and pockets of ethnic and racial hatred). Some academics, however, and anybody who has ever lived among the poor (what policy changes might a greater overlap of these two classes produce?), recognize that the dominant causal arrow in this relationship runs the other way. I certainly do not mean that all poor people are so because they (or their parents) are lazy, criminal, drug-addicted, etc. It takes only a few well-armed thugs, drug dealers, and street bums to turn a prosperous community into a wasteland of barred windows and empty streets, where bathtubs are valued as much for their ability to slow a bullet as bathe a child. The resulting environment recycles itself, as children become socialized into a life where jobs are scarce and violence is commonplace. Just as we saw above that policies can be self-reinforcing, so can destructive community norms—at least until the community self-destructs faster than it replenishes itself.

Hays seems to recognize as much when he criticizes policy makers for placing housing projects in slums, where they became loci of crime and social decay. The problem, of course, is that the poor have

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to live *somewhere*. As Hays recognizes elsewhere in his book, you can't make Americans concerned with their safety and property live close to housing projects if they don't want to. And nobody wants to—especially poor homeowners, who comprise some of the most vocal opponents of low-income housing. Thus, wherever you put the project becomes, in time, a new slum. That policy makers were in some cases foresighted enough to skip the middle step by placing projects in existing slums earns them little credit with Hays. His unwillingness to question the roots of neighborhood decay leaves him free to sidestep the possibility that placing free and subsidized housing in decent neighborhoods tips them into a downward spiral of abandonment and poverty (Schelling 1971; see also Granovetter 1978).

Paved with Good Intentions

If poverty flows from the unchecked pathological behaviors of some community members, then it is logical to ask about the origins and sustenance of these behaviors. Though there may be some truth to the theory that they stem from poverty (leaving aside the lack of empirical corroboration, poor theoretical construction, and piercing refutations that confront this hypothesis),[2] it is worth asking whether the type of interventions advocated by Hays and others might themselves have contributed to this social decay.[3] Salins and Mildner, as we have seen, provide convincing evidence that this is the case.

Hays does not consider this possibility. He reports, for example, that public housing programs penalized the upwardly mobile among their beneficiaries. He spends not one sentence, however, considering the ramifications of this incentive structure for long-term behavior. During the 1940s and 1950s, public housing residents whose incomes rose above a maximum were evicted so that only the truly poor would benefit from government housing. Similar disincentives existed in other welfare programs. For example, single mothers were eligible for AFDC, while married parents largely were not. Even though such restrictions have since been gradually relaxed, one unavoidable restriction on eligibility remains: you can't get welfare if you produce too much for society.

Hays fails to address the obvious consequences of such a system. He observes that the evictions for rising incomes were harmful because they put people on the streets, but doesn't seem to understand

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that such an incentive (and others like it) will affect behavior. An earned dollar loses its value when the combination of taxes and lost benefits results in a tax rate of 100 percent, or higher. Marriage is less attractive when benefits flow more freely to single mothers. Furthermore, monetary incentives for illegitimacy may serve to elicit behavior that erodes the moral disincentives for illegitimacy, leaving in place a perpetual cycle of poverty and illegitimacy in many neighborhoods even after changes in welfare policies remove the monetary incentives (Murray 1993; Nechyba 1996). The history of the Great Society is thus a sad tale of dependency, fostered by well-meaning social workers and academics and their often cynical political allies. Removing these policies holds no promise of reversing their damage, however, because the social pathologies they have spawned are now self-perpetuating.

Urban renewal has a similarly sordid history of destroying neighborhoods and evicting the poor (Anderson 1964) that is largely ignored by Hays. Although he discusses the difficulties confronting refugees of urban renewal in their search for other housing, he neglects the far more serious long-term effects of urban renewal on small businesses and communities, and on minority communities in particular. A closer look at the uprooting of vibrant black neighborhoods to make way for luxury hotels, civic centers, and highways reveals that, in many cases, the term "relocation" should be replaced with "dissolution" because the communities never grew back. What to wealthy white liberals appeared to be second-rate diners, shoe repair stores, and vegetable stands begging for demolition were, in fact, the building blocks for increasing job skills and prosperity among people labeled poor.[4] But bureaucrats and social workers, who mistook a dynamic system of social mobility for a static state of poverty, concluded that the only way to develop such neighborhoods is to raze them to the ground and build on the rubble, dispersing their unsavory residents in the process.

The Rest of the Story

The most disappointing aspect of *The Federal Government and Urban Housing* is Hays's poor data analysis. What is supposed to give force to his account is the fact that housing prices are going up while homeownership rates are declining, suggesting a growing gap between the

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housing haves and have-nots. The problem with assuming that rising housing prices result in fewer homeowners is that this doesn't hold quality constant. In other words, home prices might well be rising because buyers with more disposable income, facing lower interest rates compared to the 1970s, are choosing to finance larger homes with more amenities. In fact, controlling for quality reveals housing price increases to be barely ahead of—and sometimes behind—general inflation since 1969 (Abraham 1990). As for the decline in homeownership, it amounts to a drop during the 1980s of 1.7 percentage points, to 63.9 percent of all households. Hays picks a peak year for homeownership, 1981, as the starting point for his series, and stops at 1989. The homeownership rate in the year his book was published, however, was 65.1 percent in the final quarter. At the end of the third quarter of 1997 it stood at 66 percent—a record high. So much for declining homeownership.

To bolster his argument that Americans are finding it increasingly difficult to afford homes, Hays points to stagnant or declining incomes during the 1980s. But when properly measured (i.e., by individual income to adjust for changing family size, and including benefits, which have comprised an increasing portion of compensation), the average earnings of every income quintile increased during the 1980s, with the lowest growth quintile posting a 10.1 percent increase (McKenzie 1992). This fact suggests that rising home prices are due to the fact that buyers have more disposable income.

In order to make the point that black and female household heads are especially hard hit by a declining federal commitment to housing, Hays points to the large household income gaps between men and women and blacks and whites. But there has, in fact, been no decline in federal housing programs. Hays is misled in this regard by his reliance on the *proportion* of federal spending going to housing and development subsidies. Faster growth in other programs thus makes it appear that housing spending has declined, even though it has increased at an annual rate of 9 percent since 1980. As for the income comparisons, the large gap in black/white household incomes is driven substantially by the ominous chasm between the two in out-of-wedlock birthrates. While roughly 23 percent of white children are born out of wedlock, 68 percent of black children are, with the figure well over 80 percent in large cities. Thus, when Hays compares household incomes, he is often comparing the incomes of two working white parents to that of a single black parent. To make matters

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worse, since young women have the most out-of-wedlock births, Hays is comparing statistics for educated white parents to those for single black mothers with little chance of furthering their education. The education and job experience disadvantages suffered by single mothers also drive the gap in household incomes between male and female heads of household. Indeed, once we begin to control for things like single parenthood, average age, continuity in the workforce, region, and education,[5] these income gaps close almost completely.

Hays's analysis of homelessness is also incomplete. He wants to lay as much blame as possible on inadequate federal housing initiatives. This leads him to neglect ample empirical evidence that a majority of the homeless suffer from alcoholism, drug addiction, mental illness, or some combination of these (Jencks 1994). Many of these people will be no more helped by additional public housing than drunken drivers will be helped by new roads. Rather, many of the homeless are trapped in self-reinforcing systems of pathological norms and harmful policies.

Ideology, Elite Dissensus, and Compromise

The most interesting part of Hays's work is his attempt to frame his historical survey with a political model that incorporates elements of elite theory. He uses a model of subgovernments (Freeman 1965; Lowi 1969), wherein policy areas are dominated by sets of interested political and private elites. At the federal policy level, these elites include key members of relevant Congressional committees, federal agencies, and corporations and individuals with substantial interests in policy outcomes. This model diverges from the concept of an iron triangle of interest groups, Congressional committees, and administrative agencies by incorporating the refinements of Hugh Heclo's (1977, 1978) notion of "issue networks." This allows Hays to notice that among the interested elites are not only those who benefit from a program, but also academics, journalists, and others who have expertise in it. These subgovernments produce policy equilibria that are hard to unbalance, as existing groups work to exclude new groups with differing policy preferences.

These differences in policy preferences are driven by ideology, which Hays defines as a set of interrelated beliefs that guide action. Hays notes that interrelated beliefs need not be consistent with one

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another. For example, ask the average American whether the government should intervene heavily in the economy, and he will almost certainly say "no." Follow up by asking if the government should guarantee a job to every American willing to work, and odds are he will say "yes." Of course, the two answers are inconsistent, but that is the nature of public opinion, and this insight holds important implications for ideology, electoral behavior, and, ultimately, public policy.

Hays makes interesting use of belief inconsistency by applying it first to political elites. He argues that although American elites share a respect for the ideas of capitalism and democracy, they have developed differing "interpretive frameworks" for these ideas. In turn, they have gathered into two broad factions that Hays labels "liberal" and "conservative." Hays defines liberals as those who favor government amelioration of undesirable market outcomes (presumably poverty, unemployment, discrimination, etc.). Conservatives are those who favor only augmentation of market outcomes (tax breaks and subsidies for businesses). Both, says Hays, are "firmly rooted in a common capitalist world view" (8).

As for the masses, Hays writes, they harbor "a collection of interrelated images and facts" that they "associate with particular persons or events," but they have no clear interpretive frameworks, as elites do (7). In short, their views on politics, when existent, are jumbled and uninformed. This, Hays notes, leaves them "vulnerable to symbol manipulation by elites wishing to justify their policies." These masses of voters are the audience that liberals and conservatives must please as they battle to implement their visions of how government should operate.

For Hays, this ideological battle between factions, waged with rhetoric and symbolism before an ignorant public, is the cause of U.S. housing policy failures. It has produced compromise policies that are at once suboptimal and, because they are rooted in majority votes, difficult to change. Furthermore, the solidified ideological factions of elites serve to filter policy initiatives and participants, excluding those outside established policy paradigms. Ideology thus serves as both an internal and an external constraint. Mainstream elites are internally constrained by their own philosophical affinity for capitalism and democracy, and externally constrained by public respect for the symbols and vague ideas associated with these systems. Elites outside this mainstream, moreover, are externally constrained both by the lack of mass support and by the ability of centrist elites to freeze them out of the subgovernments making policy.

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Some of this analysis rings true. Much of what passes for political debate among elites is simply posturing and rhetoric designed to produce favorable public opinion. It is also true that handfuls of interested actors hold sway in most policy areas, so long as they do not create waves of public discontent (and semi-awareness) high enough to swamp the boat. Hays is clearly on to something when he decries policy compromises that cripple a program's potential effectiveness while at the same time perpetuating its tenure. In fact, when one combines inconsistent voters with compromises among policy makers, one finds a partial explanation for why the inherently self-reinforcing nature of public policy is so threatening to civil society. Not only do policies perpetuate themselves; they often start from a suboptimal or irrational point. Rather than work themselves out of these beginning "ruts" via some incrementalist process of long-run optimization, these policies merely contribute to the creation of entire subsystems of policies aimed at remedying their own ills. These subsequent policies, as the work of Salins and Mildner illustrates, often carry with them further harmful and unanticipated consequences.

Yet Hays reveals only a portion of the trends that lead to irrational, self-reinforcing policies. As a result, he misjudges the extent to which policies represent centrist compromises. In addition to his observation that legislative compromise is a product of political factions, Hays would benefit from attention to the gaps that often grow between legislation and implementation. Consider the 1990 Americans with Disabilities Act. Driven by the symbolic value of assisting the disabled, yet mired in profound disagreement over the meaning of "disability" and over what constitutes a "reasonable" burden of accommodating the disabled, the measure's key advocates in Congress adopted ambiguous language in order to secure passage of the bill. Because the Supreme Court empowers federal agencies to interpret unclear legislation, the Equal Employment Opportunity Commission (EEOC) now has wide latitude to determine the distribution of the costs and benefits created by this law. The agency has proven more than willing to pursue expansive definitions of disability, while seeking exorbitant remedies from the institutions it regulates. Compromise, in this case, leads not to centrist, majority-preferred policy, but to radical policy—a clear departure from Hays's thesis.

Even legislation that is not vague, such as the 1964 Civil Rights Act's prohibition of employer discrimination, can be bent to satisfy ideologically extreme members of the implementing agency. Despite

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very clear language prohibiting discrimination on the basis of race, the EEOC has crafted rules over a span of 30 years that *require* racial discrimination on the part of governments and publicly funded universities. The tendency of bureaucracies and courts to distort centrist legislation in the service of extreme ends has drawn growing attention (Bork 1996).

The foregoing observation raises questions about Hays's characterization of elites as supporters of capitalism and democracy. At what point does it become meaningless to say that his liberal and conservative factions (themselves questionable conceptualizations) are "firmly rooted in a common capitalist world view"? Once we acknowledge that the only true indicators of elite valuations of capitalism and democracy are their expressed policy preferences, Hays's claim appears to be in danger. How are support for racial preferences, restrictive trade barriers, government control of the health-care system, or micromanagement of agriculture rooted in a capitalist world view? How do we reconcile widespread elite support for court decisions that invalidate popular referenda on term limits, welfare, and affirmative action with Hays's belief that these same elites cherish democracy?

Hays is clearly correct that political elites frame their policy preferences using symbols supported by the masses. But this does not mean that they are *internally* constrained by a common American ethos. It is closer to the truth to say that political elites feel *externally* constrained by the opinions of the public, and by little else. Witness their escalating expenditures on polls and focus groups, all to tell them what Americans think. The goal is surely not to better serve the voters' expressed interests. President Clinton's polling team (among others) actually gives focus groups machines with dials attached while they watch clips of him speaking. Viewers are instructed to turn the dial one way when they hear something they like, and another way upon hearing something they dislike. The end product is a sort of speech EKG, which the opinion technicians use to isolate phrases and words that resonate with the audience. These words are then inserted into descriptions of proposed policies, regardless of their accuracy. Not even Orwell predicted as much.

Hays is, of course, aware of this, but his standard for classifying a policy preference as capitalist or democratic seems infinitely elastic, so that elites' use of capitalist and democratic symbols to describe their policies are merely advertising, not fraud. As proof, consider his claim that "the actions advocated by liberals, from the New Deal onward,

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have been minor modifications of market outcomes" (22). In a world in which every policy is centrist, it becomes easy to blame policy failures on compromise, suggesting that more forceful manifestations of the same programs would have succeeded.

* * *

It is tempting to think of policy failures—the broken promises, unintended harm, wasted resources—as the result of deficiencies on the part of policy makers. That view is often accurate, in fact. Salins and Mildner make a convincing case for poor economic reasoning as a cause of many of New York's preposterous regulations. Hays argues persuasively that policy makers value compromise and appearance over principle. Unfortunately, the belief that bad policy makers are the cause of bad policy effects can lead to two mistaken assumptions. The first is that employing more competent and well-meaning bureaucrats will produce better policy outcomes. The second is that eliminating the offending policy will undo its effects. Once we begin to view the web of policies and their effects as a self-sustaining system, however, the error in such thinking becomes clear. Will eliminating rent control, for example, stop the spreading zones of destruction in New York City? Will ending the subsidization of poverty eliminate its baneful effects on inner-city culture?

While we can lay blame at the feet of those who created these and other harmful programs, this takes us no closer to a solution. The dilemma that confronts those who agree more with Salins and Mildner than with Hays is this: At the end of the day we are no closer to solving the problems whose origins we believe we have correctly uncovered. That is the most damning indictment of government activism—the fact that no solution emerges from a proper diagnosis of the problem. Many conservatives and libertarians seem to believe that their mission is to educate, as if the harmful programs they decry will dissipate with the ignorance of their opponents. The harm, unfortunately, remains. And in many places, it spreads like a cancer.

NOTES

1. For a more formal discussion of the consequences of limited information and self-reinforcing policy for analysis of urban politics and history, see Woodlief 1998.

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2. See, in particular, Nettler 1974.
3. See, for example, Banfield and Wilson 1963 and Anderson 1964.
4. For evidence that many communities demolished by urban renewal were in good shape, see Husock 1991.
5. As Thomas Sowell (1995) illustrates, even controlling for education can be misleading if done improperly, because of vast differences in the kinds of college degrees earned by blacks versus whites, and men versus women.

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Alvaro Vargas Llosa

A CAPITALIST REVOLUTION IN LATIN AMERICA?

ABSTRACT: *While it is true, as Paul Craig Roberts and Karen Lafollete maintain in* The Capitalist Revolution in Latin America, *that Latin America has begun to break away from its statist tradition, the basic culture of mercantilism, corporatism, and interventionism remains, underpinned by the positivist tradition that has made public policy and legislation a substitute for the rule of law, as reflected in a schema of essential rights. The confusion between a private-enterprise economy and a free economy is at the heart of the failure of Latin America to create a truly competitive, privilege-free, and institutionally adequate economic environment, while the divorce of market economics from the rule of law has led to an authoritarianism that has undermined the transition from a state-led to a private-enterprise economy.*

In almost two hundred years of independent life, Latin America has achieved a great deal of note, but, with the exception of Argentina's extraordinary capitalist expansion between the 1870s and the 1920s, its successes have not been economic. Now, however, things look different. There is an apparent continental trend in favor of "economic modernity," there is an intellectual undercurrent sustaining this trend, there are some impressive results, and there is positive feedback from the economic capitals of the world.

Paul Craig Roberts and Karen Lafollete capture this mood very

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Alvaro Vargas Llosa is the author of *Diablo en campaña* (El Pais/Aguilar 1991), *The Madness of Things Peruvian* (Planeta, 1993), *El Manual del perfecto idiota latinoamericano* (Plaza and Janés, 1996), and *El exilio indomable* (Plaza and Janés 1998).

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well in their account of *The Capitalist Revolution in Latin America* (New York: Oxford University Press, 1997), exploring in detail the reforms undertaken in the most significant countries; discussing the ideological, political, and economic reasons this part of the world has failed, hitherto, to create prosperous societies; and lashing out with devastating force against the contribution made by the developed world to that failure. The book exudes the kind of optimism that has been (at least until recently) present in the financial institutions, the political and journalistic circles, and the business community of Latin America. My opinion, however, is that this optimism is premature, ill-informed, and superficial, and could easily become our worst enemy—a new way of encouraging the failure of Latin America.

If we consider the rule of law as the foundation of true progress, including economic progress, and view development as an institutional and cultural process of which economic prosperity is an offshoot, Latin America is very far from the point where fashionable opinion has placed it. We have not yet, in fact, even abandoned mercantilism—the system that prevailed in the Europe of the sixteenth and seventeenth centuries, in which economic progress is only possible in the orbit of the State and the economy remains the fiefdom of a privileged elite. Our societies still treat the law as an instrument of power—a power whose authoritarian nature varies from place to place, but is present, in some form, almost anywhere. Failure to see this amounts to mistaking a private economy for a free one. If having fewer public enterprises, reducing inflation, and letting in private foreign investment were enough to bring about stability and prosperity, Latin America—which has grown on average 5 percent every year since 1940, or more than Europe in the same period—would have been prosperous a long time ago.

One must, of course, begin by recognizing that something is changing, that not every reform is a mirage, and that a certain set of ideas, a certain mentality, which accounts for its chronic economic failure has been partially defeated in Latin America. The statist, corporatist, and socialist traditions have suffered a heavy blow. That, in itself, is quite an achievement, because these traditions are very old. They did not start in the 1960s and 70s with the massive wave of nationalizations; they were present in ancient cultures—the Inca state was in many ways a totalitarian one—they were present in the colonial period, and they lived on after independence, whose heroes liber-

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ated the region from Spain but not from a centralized way of organizing political and economic institutions.

As Roberts and Lafollette quite rightly recall, economic activity in the Spanish colonies was devoted to "rent seeking"—the very antithesis of capitalism. There were no real markets for commodities or manufactures, although there was a market for public offices, which were bought and sold openly. Every sector of the economy was under a monopoly or some form of protection—gold, silver, sugarcane, cacao, wood, in Latin America; and olive and textile products in Spain itself. There was also, of course, slavery and forced labor, and, as is inevitable in any such economy, a huge underground sector (by 1686, contraband represented two-thirds of colonial commerce). Independence compounded these problems with new ones. Military values were strengthened; authoritarianism and nationalism combined to prevent the emergence of free institutions and to perpetuate the rent-seeking mercantilist system. Fashionable Comtean positivism spread throughout the continent, and from it was derived the notion that the government had the moral responsibility for the well-being of the people, to be achieved by means of social engineering. Even liberals—in the classical sense—were impregnated with Comtean positivism, and its offspring, juridical positivism, and assumed that the deliberate actions of "liberal" governments, and not the spontaneous and voluntary action of liberal societies, would bring about progress (Véliz 1994).

One answer to the question of why independence was economically so unsuccessful came in the form of a critique of the Spanish inheritance. Francisco Bilbao, an anti-Catholic Chilean writer, argued that progress would only come if the new republics broke with the inflexible, authoritarian, and corporatist colonial tradition (Bilbao 1897-98), a perspective that was developed in Argentina by Faustino Sarmiento in the second half of the nineteenth century. Sarmiento ([1974]) thought the cultural factor was decisive in Latin American underdevelopment, and added to his condemnation of Spanish influence a critique of the institutions and customs of the native Indian population. To borrow Claudio Véliz's recent dichotomy, Sarmiento proposed substituting the "Gothic fox" for the "Baroque hedgehog" model of society (Véliz 1994).

The man who produced the single most important Latin American political work of the nineteenth century, Juan Bautista Alberdi, was also responsible for the prosperity the Argentinean population en-

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joyed from the 1870s to the 1920s. His *Bases y puntos de partida para la constitución política de la República Argentina* (1852), which became the basis of the 1853 Argentine constitution, is a work of prodigious lucidity about the kind of political and economic institutions that form the basis of civilization and prosperity. Although demography loomed large in his work (Alberdi believed it was necessary to populate the underpopulated countries of Latin America with European immigrants), the essence of his thought lay in his understanding of free institutions. He wrote with irony that "glory is the plague of this poor Latin America of ours," and then, in what was a sacrilegious questioning of the values of the independent republics, predicted: "The fatherland owes much to its noble hearts and to those spirits highly cultivated in moral sciences; but it will owe more, in future, in economic matters, to simple traders and practical economists, born out of the world of business" (Alberdi 1852, 38).

Unfortunately, Alberdi was not heeded outside of Argentina—and even there, his compatriots eventually threw away the enormous progress brought about by the triumph of his ideas in the previous century. In their place grew the flattering notion that Latin America constituted a kind of spiritual reserve against the materialism of the United States. The French-born author of *Del Plata al Niágara* ([1980]), Paul Croussac, and, more famously, Uruguayan writer José Enrique Rodó, author of *Ariel* (1900), inaugurated a rapprochement with the Spanish inheritance, finding in it a bastion against the perverse influence of the North. Their followers built a new, anti-imperialist *Weltanschauung* that was to dominate Latin American culture in the nineteenth century and provide, in its degenerate form, the intellectual underpinning for the anticapitalist states of the region. Uruguay stands out for promulgating the first "Latin American welfare state" in the late nineteenth and early twentieth centuries.

It was the Mexican Revolution, however, that became the focal point of anti-imperialist thinking and provided the agrarian outlook that was to dominate revolutionary and "progressive" intellectual life, imbued as it was with nationalism. This influence is still present today in Mexico, where "modernization" coexists with the idea that the nation-state is sacrosanct. The Bolshevik Revolution also served as a beacon that gave a Marxist direction to Latin American thought. Two Peruvians—José Carlos Mariátegui (1976), on the Marxist side, and Victor Raúl Haya de la Torre (1976–77), on the socialist and national-

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ist side—exercised a significant influence on the region's political outlook in the first half of this century. They symbolized, in many ways, the ideological empire of the left in its two guises. Marxism and socialist nationalism dominated the consciousness of many Latin American intellectuals and governments of both the left and the right for decades. With the exception of the Venezuelan Carlos Rangel, author of *Del buen salvaje al buen revolucionario* (1976), there has been, until recent years, a notorious absence of thinkers on the side of both the rule of law and free-market capitalism in Latin America.

Given this tradition, we can only view as something extraordinary the current movement against statism in Latin America. The administrations of, among others, Alan García in Perú, Daniel Ortega in Nicaragua, Siles Suazo in Bolivia, and Raúl Alfonsín in Argentina (a man who is, nonetheless, rightly credited with the triumph of political democracy in his country) created the kind of massive social frustration from which radical political change is born. And violence helped push people's desperation to the limit, making radical change easier to implement.

In the last few years many governments have privatized state-owned companies, brought down trade barriers, liberalized various aspects of the financial, commercial, industrial, and agricultural sectors, and undertaken more disciplined fiscal and monetary policies than in the past. The average rate of inflation in the region was 17.7 percent in 1996, compared to 200 percent in 1991, and per-capita income has increased every year since 1990 (except for one year), while it fell by 10 percent in the 1980s. In 1996, the net inflow of private capital amounted to almost 50 billion dollars. Of the top 500 companies, currently 148 are foreign owned, 285 are owned locally, and 67 belong to the state sector. Five years ago, 140 were foreign-owned, 268 were owned locally, and 92 belonged to the state sector, which means that now, more foreign companies are doing well, more local companies are doing well, and the state sector is becoming less significant. The fact that no more than eight of the top 20 companies deal in natural resources is a sign that there is some diversification, as well (see *América Economía* 1996/97, 92; Edwards 1997, 3–5; *Financial Times* 1997; Linowitz 1997). Mexico, Chile, and Argentina—the three countries on which *The Capitalist Revolution* chooses to focus—have made impressive moves toward privatization. There were 1,155 state-owned companies in Mexico in 1982, and half of the country's production was in the public sector. By 1993, $24 billion worth of state assets had

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been sold and the corporate and maximum individual tax rates had been brought down to 35 percent, a reasonable level by international standards. In Chile, privatization—which started much earlier than in the rest of the continent—has revolutionized "untouchable" sectors such as pensions and health. Almost $25 billion worth of retirement funds have boosted savings rates, expanded the local capital base dramatically, and brought enormous improvements in people's retirement benefits, which have increased their value by 14 percent per year since 1981. More than one-third of the workforce has acquired private health insurance after the state gave people the choice of opting out of the state system, requiring only that they contribute a minimum of 7 per cent of their salary to private insurers. And other countries have also privatized. In Peru, almost a hundred companies have been sold off, a figure approaching half the number of state-owned companies under the socialist military dictatorship of the 1970s (Linowitz 1997, 8-17).

Almost a decade after this process began, however, both the results and the quality of the reforms leave much to be desired. Latin American economies grew an average of 3 percent a year in this period, three times the level of the 1980s but half the level of the 1970s and much less than the 8 percent-a-year level of East Asia. Only Chile and El Salvador have performed much more strongly than the regional norm. In most countries, real wages are lower than they were in 1980. Unemployment has increased since 1991 in (*inter alia*) Mexico, Argentina, and Peru, although Chile has brought down its unemployment figure to 4.5 percent. The number of very poor people, who live on less than $1 a day, has increased since 1990 in Latin America from 23 to 25 percent, while in East Asia it has fallen from 17 to 13 percent (Linowitz 1997, 8-17).

Foreign investment in Latin America is at around 20 percent, which is nearly half the level of East Asia. While foreign investment is at record levels, very little of it is in the form of direct investment, since investors are sufficiently insecure about Latin America to prefer readily liquid forms of investment there. Direct foreign investment levels are about a third of those registered in East Asia. The volatility of speculative investment was seen clearly during the peso crisis in Mexico at the end of 1994 and, more recently, in Brazil, in the third week of July 1997, when the Rio stock exchange fell by 16 percent (Linowitz 1997, 8-17). Latin America has begun exporting more, but not nearly enough, especially if we take into account that a small

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economy grows at about half the rate of its exports. And the region is still heavily dependent on natural resources (this is true even of Chile).

What accounts for this relatively unimpressive performance may be a conflation of private enterprise with free-market capitalism, and of macroeconomic management with the rule of law. It is true that Mexico has distanced itself considerably from the policies of Lázaro Cárdenas and other famous populists of the PRI. But what kind of "capitalist" revolution could take place within a political structure that is as authoritarian as ever and in which, for instance, the $24 billion of state subsidies channelled by the Banco Nacional de Crédito Rural to peasants over the past 20 years has only strengthened local bureaucracies and corrupt politicians, sowing the seeds of guerrilla violence? In Argentina, Carlos Menem has gone a long way toward getting rid of the heritage of the founder of his party, Juan Peron. But in hailing Argentina's reforms Roberts and Lafollette seem to forget that public spending has gone up by 100 percent since the beginning of the 1990s, forcing the new Finance Minister, Roque Fernández, to undertake a "shock" therapy that causes the popular mind to equate free markets with sudden price increases of 40 percent for diesel fuel and 10 percent for oil (Bongiovanni 1996, 13). Mexico's bottomless pit of rural subsidies and Argentina's huge public expenditure exemplify how incomplete reform has been and how little it has displayed the overall coherence that could come only from awareness of how free societies work.

Privatization is, in fact, often nothing more than the replacement of public monopolies with legally enforced private ones. The ownership of certain sectors of the economy has been transferred to private hands while markets remain protected, fueling corruption and scandal. When the state divests itself of ownership but continues regulation, the result is usually not free competition but new opportunities for profitable deals of a mercantilist kind and for enhancing state revenues. People in Peru understandably complain about the huge telephone bills and poor service brought about by privatization of the telephone company, which continues to be protected from competition. In Argentina the state telephone monopoly was merely broken up into regional private monopolies. In Buenos Aires public transportation is now privately owned, but each route is monopolized by one company, and state-enforced monopoly is now equated with a free market.

Around the continent, meanwhile, privatization is not even con-

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templated in certain "strategic" sectors, such as oil in Mexico and Venezuela. Roberts and Lafollete give a devastating account of the way the PRI corrupted the state-owned Mexican oil company, PEMEX, from top to bottom, yet they consider the continued existence of PEMEX a small matter that does not bring into question the "capitalist revolution" in Mexico. Venezuelans, for their part, have not benefited in any way from the phenomenal $250 billion generated in the last 20 years by their state-owned oil company, yet the suggestion of privatizing it is met with hysteria in that country. Even in Chile, the "strategic" value of copper keeps that industry, a sort of national totem, in state hands. In Brazil, the economic giant of Latin America, there are still almost 150 companies in public hands. President Cardoso has been much busier trying to amend the Constitution to get himself reelected than privatizing companies.

Even where privatization has occurred, moreover, ownership has been concentrated in a few hands. Whereas in Great Britain and, more recently, in Central European countries, privatization was used as an opportunity to spread share ownership widely among the population, in Latin America, workers have by and large been excluded from the process. Apart from the benefits workers may have obtained as consumers in terms of some quality improvement, their main relationship with the process of privatization has been unemployment and higher prices. In Peru, one in four people have lost their jobs as a result of privatization, and "capitalism" has come to mean little more than mass layoffs. In the absence of open and competitive markets—the prerequisite for the huge advantages of privatization—privatization has served primarily to relieve the state of money-losing enterprises and to multiply the forms of corruption.

The perception that liberalization means giving out privileges to the rich and powerful is spreading throughout Latin America. Free-market capitalism, which is in its very essence the negation of privilege and which disperses power throughout society, forcing even the most successful people to serve social needs or lose income, is beginning to look, in the eyes of ordinary Latin Americans, like the preserve of an oligarchy. Of the 15 Mexicans featured in *Forbes*'s 1996 list of the richest people in the world, there is not one who has not benefitted from state-granted privileges, whether financial, commercial, or monopolistic. In Colombia, FEDERCAFE, the national federation of coffee growers, is so intertwined with government—it is even represented in the executive branch—that it determines policy, as Roberts

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and Lafollete point out quite convincingly while extolling the "capitalist revolution."

Above all, institutional and structural reform have been lacking. Latin American legal codes are replete with regulations specifically designed to benefit some at the expense of others. The result is that many Latin Americans can only engage in economic activities illegally, producing a huge "informal" economy. In countries like Peru this economy comprises half the country's working population. In every area—from commerce to urban development—the cost of the law is such that the law becomes a privilege in itself. Registering a small manufacturing workshop in Lima takes 289 days, costs more than $1,200, and requires several bribes, whereas this process takes place in Tampa, Florida, in a matter of 3 hours. Once a small industrial company is registered in Peru and is operating legally, complying with regulations, taxes, and bureaucratic procedures costs three times the average profit. A fifth of this cost goes toward paying myriad different taxes, but nearly 70 percent goes toward obeying regulations. If all of the creative energy and entrepreneurial drive that constitutes the "informal" economy instead operated inside the legal system, it has been suggested that GDP would grow by more than 50 percent (de Soto 1989).

In Peru and other Latin American countries, the positivist confusion between law and legislation has meant that in practice, whatever the constitutions say, there is no basic law to preserve essential rights—only legislation that changes according to the whim of those in government, both those at the very top and those in the lower ranks of the executive branch (98 percent of legal rules are promulgated by the executive power in Peru, and just under 2 percent by Congress) (92–93). Every norm, every piece of legislation produces privilege and discrimination. The particularization of law extends beyond the legal protection of such private commercial monopolies as Telefónica and Aeroperu in Peru. Less visibly, the Peruvian National Assembly of University Presidents, for example, has been given such powers that potential competitors to the existing universities are severely hindered and often totally prevented from establishing themselves.

Douglass North and Robert Paul Thomas (1973) have demonstrated the impact that the establishment of security in property rights, contracts, civil liability, and the administration of justice had on the Industrial Revolution. Latin America has seen no such change,

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which would amount to a true revolution. The corrupt and politically manipulated Latin American system of justice remains in almost the same state it was in a decade ago, when reform began.

It should be noted that the rule of law is not the same thing as democracy, and that the latter can be as much of a threat to the former as can authoritarianism. Indeed, authoritarianism and democracy often work together. In Peru, the authoritarian government was able to bolster its popularity by imposing price controls on electricity. This action slashed the expected profits in this industry by 40 percent overnight. Prices that were artificially high because of monopoly conditions suddenly became artificially low because of price controls. Not only does this incident typify the process by which new forms of state control are added in compensation for the failure of previous forms; it illustrates the economic instability that flows from the confusion of changeable, particular public policies with firmly protected, universally applicable general laws. In Argentina, a number of industries were exempted by the reformist government of President Raúl Menem from paying the VAT, while others had to pay as much as 21 percent. Suddenly, in 1998, the exemptions were repealed and new ones were created. No coherent explanation was given and the economy was thrown into confusion. This sort of thing happens, of course, in all the nations of the world. But in Latin America there is little pressure against such arbitrary expressions of authoritarian or popular will, because there is neither an independent, uncorrupted judiciary nor the intellectual background that would make the rule of law a compelling ideal, however much it might be eroded in practice.

A further effect of the absence of the rule of law is the scarcity of personal and property rights and the vulnerability of those that exist to being withdrawn outright—not merely overburdened with regulations and taxes. As Hernando de Soto (1989) has shown, during the 1980s, Peruvian peasants silently privatized 60 percent of the land that had been collectivized by the socialist agrarian reform of the seventies. Yet property titles still have not been conferred on the 4 million *parcelero* families of the Peruvian Andes. At the other end of the socioeconomic spectrum, in 1997, Baruch Ivcher, a Jewish businessman, was stripped of his Peruvian nationality by Fujimori's government in order to expropriate his TV station, which had just revealed that the phones of 200 prominent Peruvians, including many pro-Fujimori businessmen, had been tapped by Fujimori's intelligence service. No-

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body is safe in Peru because no rights are treated as immune from being overridden by the state.

In Mexico, the much-welcomed reform of the collectively-owned *ejido* has not translated into property rights for peasants. By 1991, thanks to Carlos Salinas's "Solidarity" program, 63 percent of village governments had their own public works budget. The Chiapas revolt indicates that this money did not go to peasants but to *caciques* of the ruling party. According to the Mexican economist Oscar Vera Ferrer (1987), CONASUPO, the agricultural supply company, channeled only 14 percent of government subsidies to those for whom they were intended. PRI reforms have not touched the *cacique* structure, which accounts for a great portion of Mexico's underdevelopment. In 1992, Salinas amended the Constitution to allow private companies to own and develop rural land for agricultural purposes. But firms were limited to owning no more than 2,500 hectares, and the state was still the real owner of the land. Article 27 of the Constitution states that the "nation" is the collective owner of land and water. Private property thereby becomes a concession, not a right. Article 25 gives the state the right to "plan, conduct, orient and run" the economy. No wonder most foreign dollars go into short-term, speculative investments.

The superficiality of Latin America's so-called capitalist revolution shows that political reform is the foundation of economic reform. The political and institutional framework is the context in which the economy, whether closed or open, operates. The historical roots of capitalism are, of course, subject to intense disagreement, but it can safely be stated that in the most successful capitalist countries the rule of law, often a byproduct of political decentralization, was a precondition of economic development. It is also clear that politics very soon becomes an obstacle to the growth and functioning of capitalism in authoritarian or populist environments. This has proved to be the case in East Asia, where the recent financial crisis can be read as a symptom of the weakness of market economies that operate in relatively closed polities, but we also have good examples in Latin America. Chile's economy was never as robust, stable, and confident under Pinochet as it has been since the democratic transition almost a decade ago. The corruption, abuse of power, and loss of business confidence evident in Peru indicate how dictatorship, which is by definition the opposite of the rule of law, becomes the worst enemy of capitalism, which hinges on the security of property and person. Peru's stock exchange fell 5 percent the week that Fujimori stripped Ivcher

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of his nationality. One could not ask for a better emblem of the connection between capitalism and the rule of law.

When Roberts and Lafollette claim that one of the lessons of Latin America is "the importance of promoting effective economic reform ahead of political reform in order to protect the former from special interest groups," they are getting things backward. Where people can be exiled, imprisoned, tortured, and even killed by the state and where the law is used to benefit some interests and discriminate against others, economic reform can hardly be protected from special interest groups. In most of Latin America, the real trust that is foundational for thriving economies continues to be threatened by the control of the state by interest groups that can bend the rules the way they like. Roberts and Lafollette would do well to remember the dinner party at the mansion of Ortiz Mena, in February of 1993, at which President Salinas (endlessly praised in *The Capitalist Revolution*) asked 30 top Mexican businessmen for political contributions, resulting in pledges of $25 million each. Present at that dinner were some of the main beneficiaries of the transfer of monopolies to the private sector, including the man who won the bid to privatize the national telephone monopoly (Oppenheimer 1996, ch. 5). The absence of political reform in Mexico, as in other parts of Latin America, has meant that economic reform, while expanding the scope of the private economy and reducing that of direct state intervention, has also multiplied the mercantilist practices that Roberts and Lafollete blame for Latin American misery. If capitalism simply means private ownership, then the notion of a capitalist revolution in Latin America makes sense. However, if we define capitalism as a system of private enterprise and individual rights in which the market and not the government defines economic success, then this notion is grossly wide of the mark.

The Capitalist Revolution in Latin America quotes Douglass North's definition of institutions as "the rules of the game." These include the law, the cultural attitudes, and the other constraints that form the framework of human interaction. "The path of institutional change," North argues, "determines the level of economic opportunity in a society" (quoted, 6). But Roberts and Lafollete seem to understand "institutions" as primarily cultural traditions, not political ones. Thus, they point out that "the region is at last shedding the cultural attitude hostile to commerce, trade and work that made it more socially acceptable to court privilege and peddle influence than to compete in markets" (6), as if this itself signals the end of Latin American eco-

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nomic stagnation. To believe this is to overlook the source of the old, antimarket cultural attitudes in a political system that rewarded nonmarket, rent-seeking behavior. That Latin Americans can overcome such attitudes even in the face of the institutions that spawned them is evident from the fact that, in the informal Latin American economy, tens of millions of people have created a market system in spite of the law. Even though people may want government to redistribute pieces of a pie they assume already exists and thus does not need to be baked, as it were, by civil society, most people have come to realize from personal experience that redistributive wishes produce little in the way of tangible benefits for them, and that in practice the state is a barrier to their efforts to improve their position. Many, therefore, choose to become illegal entrepreneurs. Today, the survival of more than half of Peru's population rests on the shoulders of such furtive capitalists (93). But in thus throwing off old cultural "institutions," the politically powerless capitalists of Latin America have not managed to dislodge the political institutions that fostered these attitudes in the first place.

These political institutions contradict the rule of law. The rule of law provides guarantees of institutional impartiality and personal protection that make it possible to embark on the long-term investments that produce economic growth. Bolivia and Costa Rica, prime economic success stories in recent Latin American history, have successfully encouraged both foreign and domestic investment that has reduced poverty. Arguably the chief reason is that constitutional democracy in these countries has produced trust and long-term stability. Elsewhere in the region, however, Simon Bolivar's assertion that "the new states of formerly Spanish America need kings with the name of presidents" (quoted in Alberdi 1852, 72) expresses the spirit of Latin American politics. Until this changes, there will be no capitalist revolution deserving of the name.

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Lowell Gallaway and Richard Vedder

THE STATE AND LABOR IN MODERN AMERICA

ABSTRACT: *One of the distinctive developments of the postwar era in the United States has been the relative decline in the economic significance of labor unions. Melvyn Dubofsky offers the hypothesis that this has resulted from a shift in public policy that represents a return to pre-New Deal notions of the proper relationship between government and unions. But a much more likely explanation lies in the changing nature of American life. Increases in education and advances in transportation and communications have weakened greatly the traditional worker-versus-capitalist paradigm that is at the core of Dubofsky's thinking. As this has happened, labor unions have increasingly become relics of a previous age.*

Melvyn Dubofsky's *The State and Labor in Modern America* (Chapel Hill: University of North Carolina Press, 1994) is very much in the left-wing mainstream of labor histories written over the past half century. It is highly readable, and his central premise is one that must, at least in part, be accepted: The rise and fall of organized labor organizations in America is closely associated with governmental policies and behavior. When government favors and promotes unions, they grow. When government shows hostility (or, we would argue, even

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Lowell Gallaway and Richard Vedder, the Edwin L. and Ruth E. Kennedy Distinguished Professors of Economics, Ohio University, Athens, OH 45701, telephone (614) 593-2040, telefax (614) 593-0181, are the authors of *Out of Work: Government and Unemployment in Twentieth-Century America* (Holmes & Meier, 1993).

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neutrality) toward them, they tend to decline. Dubofsky is absolutely correct in proposing that the heyday of American trade unions (1930 to perhaps 1960) was, in general, a period when the federal government successfully used its coercive abilities to enhance the power and size of the organized American labor movement.

Workers vs. Capitalists

Dubofsky continues a tradition of labor historiography begun by John R. Commons (1918 and 1935) three generations ago.[1] In this tradition, there are two classes of people: "capitalists" or "employers" on the one hand, and "workers" or "laborers" on the other, with unions serving as the agents of the latter. Not until the next-to-last page of text (237) does Dubofsky acknowledge that most Americans identify themselves "neither with capital nor with the working class, preferring to see themselves as a protean 'middle class.'" Much to Dubofsky's chagrin, America, as Tocqueville observed more than a century and a half ago, is not a class-oriented society. Accordingly, even at the peak of union power, there were far more "workers" who labored outside unions than inside. To consider the AFL-CIO to be synonymous with working Americans, as Dubofsky does continuously throughout this book, misrepresents reality.

A good example of Dubofsky's misuse of the word "worker" comes early (4): "By the 1890s . . . the strike had become the workers' chosen weapon in their ongoing struggle against capital." In reality, most "workers" in the Gay Nineties were not "struggling with capital." Even late in the decade of Grover Cleveland and William McKinley, union membership was well under 3 percent of total employment, and under 5 percent of the number of nonagricultural workers (U. S. Department of Commerce 1975, 178). In 1894, the year with the most widespread strike activity in the nineteenth century, substantially less than 5 percent of the working population engaged in a work stoppage (Perlman 1950, 136). It is hard to believe that if workers had been able to vote by secret ballot on collective bargaining, more than a small percentage actually would have chosen union representation.

Nor does Dubofsky come to grips with changes in American life that have made organized labor a collection of increasingly irrelevant, anachronistic organizations.[2] American individualism, structural

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changes in the U.S. economy (e. g., the trends toward service employment and greater female labor-force participation), and increased global competition have led to a precipitous decline in the proportion of the labor force in unions, something Dubofsky laments but says relatively little about—possibly because it is not consistent with his view that since unions are obviously beneficial for their members, any membership decline must be due to governmentally enforced or inspired acts designed to constrain labor.

Further, it does not seem to have occurred to Dubofsky that when something becomes more expensive, people may want to buy less of it. Thus, he overlooks the hypothesis that organized labor may have been *too* successful, pushing wages up to levels that encouraged the substitution of capital for labor and the migration of businesses to cheaper, nonunion locales, both within and outside the United States. Similarly, economist Charles Baird (1996) has argued that when the legislative environment becomes extremely pro-union, at some point the result is to *reduce* the proportion of workers in unions. Baird suggests that below some threshold, the gains to unions from a more favorable legal environment serve to enhance membership, but as membership grows, a point comes at which employers, responding to the higher costs associated with a unionized labor force, begin to fight the unions more fiercely in representation elections.

Dubofsky appears to accept dubious economic arguments for labor unions, based in part on the theory of monopsony. According to this theory, when a single powerful employer of labor services negotiates with a multitude of competing workers, it is likely that workers will be compensated less than would be the case in a purely competitive market. Under monopsony, government promotion of unions may be justified, enabling labor monopolies (unions) to slug it out with monopsonists (employers) in local labor markets.

The monopsonistic model may make sense where there are company towns with one large employer hiring unskilled workers who have few other work options because of high transportation costs. One might argue that those situations were moderately common in the late nineteenth and early twentieth centuries, although our own research suggests that as early as the 1830s, the monopsony power of employers in New England mill towns was modest at best (Vedder, Gallaway, and Klingaman 1978). In any case, monopsony is far rarer today. The proportion of Americans working for distant corporate behemoths has declined sharply. For example, the percentage of workers

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employed by Fortune 500 corporations today is half what it was a generation ago. The proportion of manual workers has declined from about half of all nonfarm employees around 1930 to about a quarter of them today. In 1900, when labor-management strife was high, only about 10 percent of workers had professional, technical, or managerial jobs; today, 30 percent do—far more than have factory jobs. More education; near-universal automobile transportation; and radio, television, and computer communication have made it much easier for employees to escape from anticompetitive labor markets. In short, workers do not need unions as much as once may have been the case.

In any case, the old capitalist-worker distinction, greatly overdrawn by labor historians such as Dubofsky, is now more inappropriate than ever. Millions of Americans are *both* workers and capitalists. By the early 1990s, over 11 million workers were in Employee Stock Ownership Plans, and a far greater number were indirect capitalists through their pension-fund investments. Membership in private pension plans grew from 2.7 million in 1935 to over 77 million in 1990.

Moreover, the disparities in wealth that gave rise to resentments and class thinking have declined over time. Compare the richest American early in this century, John D. Rockefeller, with his contemporary counterpart, Bill Gates. At the time the Standard Oil Trust was broken up in 1911, Rockefeller's net worth was equal to about eight or nine days of output from the American economy. Bill Gates, the richest American today, has a net worth approaching two days of output—proportionally 75 to 78 percent less than Rockefeller's. The taxes and regulations of the welfare state have had a leveling influence that negates much of the psychological *raison d'être* of trade unionism.

In addition, today's billionaires do not run sweat shops to support conspicuous consumption. Microsoft's employees are prosperous; some two thousand of them are millionaires in their own right, as a result of stock options and stock purchase plans. The late billionaire, Sam Walton, drove his own pickup truck to work, and super-billionaire Warren Buffet lives in a house in Omaha that is less impressive than the stables in some of the Vanderbilt mansions of a century ago. Unlike three generations past, the employees of today's wealthy capitalists work eight hours a day, five days a week in air-conditioned comfort, with lots of vacation time to enjoy a plethora of material possessions, ranging from automobiles and boats to personal computers. For all these reasons, envy and resentment are not as strong as they were generations ago.

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It is no wonder, then, that American "workers" increasingly accept the proposition that their best friend is the American (or, in some cases, Japanese) capitalist. When profits are high, job opportunities tend to abound, and consumption spending by employees is higher than when profits are low. The adversarial zero-sum society is increasingly being replaced with a milieu where employee-worker distinctions and interests are merged or blurred. What is good for General Motors, by and large, *is* good for America, and most Americans know it, including many members of the United Auto Workers.

Workers vs. Unions

Indeed, the evidence is mounting that the labor unions are worse than anachronisms—that their adversarial attitudes have spillover effects that hurt innocent third parties. If the United Auto Workers prices its members out of the Michigan labor market and mandates union rules that constrain productivity, the resulting unemployment hurts retailers and their employees in the Detroit suburbs, who make a living serving the auto workers. We have used econometric techniques to evaluate variations in the economic performance of the 50 American states (Vedder 1996; Vedder and Gallaway 1986). One variable included in our models measures the incidence of labor union membership in a state. Consistently, we have observed that economic growth is negatively correlated with union membership.

Simple, descriptive evidence, while not conclusive, makes the point lucidly. For example, we compared the economic growth experience from 1980 to 1995 of the 10 states with the highest proportion of union membership against that of the 10 states with the lowest proportion.[3] The ten states with low union membership had an average rate of economic growth about 30 percent higher than the ten high union-membership states (29.43 percent versus 22.69 percent). High union membership seems to mean low growth in real per-capita income. Even while they may prompt the replacement of unskilled labor by machines, unions scare away capital resources vital to expanding output and job opportunities for high-wage laborers. Still other evidence suggests that labor unions increase unemployment; other things equal, states with substantial unionization tend to have higher average or "natural" rates of unemployment (Vedder and Gallaway 1996).

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Perhaps an even more devastating portrayal of the negative spillover effects of unions is found in statistics on human migration. If people tend to move away from a locale, it is a good sign that the overall quality of life in that area is relatively unappealing; in-migration would seem to indicate that an area has a high appeal to the populace. This is why East Germany built the Berlin Wall. During the early 1990s, more than 1.4 million native-born Americans moved away from the highly unionized states, while migrating in even larger numbers into the states with a low union presence.[4] It is hard to reconcile the traditional labor historian's view that unions protect oppressed workers from capitalistic exploitation with the fact that in the early 1990s, one thousand Americans were moving into the states with the least union presence every day.

In telling his tale of the interaction between unions and government policy, Dubofsky makes many highly questionable empirical claims. Before the reader even gets to page one, he learns that "it is no coincidence that the precipitate decline of the American labor movement coincided with the presidency of Ronald Reagan" (xiii). The reality is a bit different. Union membership as a proportion of non-agricultural employment had already declined significantly before Reagan assumed office, dropping (for example) from 30.8 percent of employment in 1970 to 25.2 percent in 1980 (U. S. Department of Commerce 1982-83, 401). And the decline continued *after* the Reagan era. To say that it "coincided" with Reagan's presidency is a misreading of the evidence.

There is a similar distortion in Dubofsky's description of the New Deal era. He claims that economic recovery in 1933 resulted in large part from the National Industrial Recovery Act: "The economic recovery precipitated by the NRA's promise of higher prices brought an increase in union membership" (116). In fact, the evidence is overwhelming that the NRA retarded recovery. Elsewhere, we have estimated that the national unemployment rate fell by 5 points (from about 28 to 23 percent) from March to July 1933—just before the NRA was implemented (Vedder and Gallaway 1997, 77). In the next two years, the rate fell only 2 more points; the recovery largely stalled, only to pick up again after the Supreme Court ruled the NRA unconstitutional in *Schecter Poultry v. United States*.[5] The high wages required by NRA industry codes priced labor out of the market.

Perhaps less egregiously, Dubofsky blames the 1937-38 downturn exclusively on President Roosevelt's contractionary fiscal policy (138,

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149). In doing so, he ignores Milton Friedman and Anna Schwartz's argument that this downturn was caused by Federal Reserve monetary policy. Dubofsky also ignores the claim made by business leaders of the era that this recession-within-the-Depression was due to the crisis of business confidence inspired by the New Deal. Least of all does Dubofsky consider the role that may have been played by the double-digit percentage increase in factory wages following the unionization of the mass production industries in 1937. This increase led to a sharp reduction in the quantity of labor demanded. Contemporary observers argued that labor unions were pricing workers out of the market (see Weinstein 1980 and Vedder and Gallaway 1997). The unions' newfound power resulted from the Wagner Act, which Dubofsky praises lavishly.

The 1937-38 experience appears to show that while unions can manipulate the supply of labor, they cannot repeal the law of demand. During most of 1936, steel industry employment had risen by over 30 percent, because real wages, adjusted for productivity growth, actually fell, making it more lucrative for steel companies to hire workers. From the fourth quarter of 1936 to the second quarter of 1938, however, wages were raised by 22 percent on average—and steel industry employment fell by more than one-half (*New York Times* 1938, *Literary Digest* 1937). This wage increase, and the resulting unemployment, reflects the United Steel Workers Union's success in organizing the industry.

Toward a New Labor History

If, as Dubofsky assumes, labor unions protect the "working class" against greedy "capitalist" employers, hostile public policies must be responsible for the recent decline in strike and union activity:

> Public policy and practice . . . resemble more the world of the late nineteenth century than that of the New Deal order, as politics and law once more privilege individualism and define militant collective action as unacceptable, "un-American" and illegal. (231)

In reality, however, recent public policy has been, at most, neutral toward unions, essentially maintaining the legal environment established in the 1930s and 1940s. When Caterpillar workers went on

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strike in the early 1990s, the government did nothing to aid either the company or the unions. When McDonnell Douglas machinists went on strike in 1996, the state of Missouri gave unemployment insurance benefits to the workers, a strongly pro-union move. Other than during the PATCO strike in 1981, no president for many decades has tried to crush a strike or discourage workers from unionizing. Indeed, President Clinton and Labor Secretary Robert Reich seemed to do everything possible to strengthen unions, even issuing an executive order of dubious legality forbidding government contractors from hiring replacement workers. George Bush, at best, halfheartedly enforced the *Beck* decision, which would have restricted the use of union dues for political purposes. During the administration of Ronald Reagan, Congress enacted the Worker Adjustment and Retraining Notification Act (1988), giving workers the right to prior notification of plant closings.

All this is merely an extension of the policies of the 1960s and 1970s. During those decades, Democratic presidents sympathetic to labor (Kennedy, Johnson, and Carter) were in power for a majority of the time, and the three Republican presidents (Eisenhower, Nixon, and Ford) were all moderates who did very little to change the legal or political environment in which organized labor operated or to curb the pro-union policies of such agencies as the National Labor Relations Board. Indeed, during the early Carter Administration, strong pro-labor legislation would have been enacted except for a Republican filibuster in the Senate. Yet during these decades, union membership fell significantly as a share of the labor force.

Dubofsky's claim (228) that President Reagan's 1981 firing of striking air traffic controllers was a brutal action reminiscent of nineteenth-century state repression of unions is certainly subject to debate. The controllers were government employees charged with promoting public safety who were expressly forbidden from striking by law, and while it is difficult to envision any president of the 1950s or 1960s doing what Reagan did, by his day Americans had lost much of their sympathy for a movement whose postwar leaders were known mainly for their corruption and their ostentatious conventions in South Florida pleasure spas.

PATCO aside, however, government action against organized labor in the contemporary United States has been distinctly muted. No major laws or judicial decisions have constrained labor. No president has used the armed forces or the courts to constrain union behavior, as

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was the case a century ago. Yet unions now claim the membership of a smaller proportion of the private-sector nonagricultural labor force than was the case in 1920, more than a decade before the passage of the Wagner Act. It is only a slight exaggeration to suggest that, absent government *protection*, unions today would be the declining remnants of a fairly vibrant type of early-twentieth century associationism, much like the Odd Fellows or the Rotarians.

NOTES

1. The citation of the Commons work is complex. It was he who wrote the first two volumes, which were published in 1918. Two additional volumes dealing with the period 1896 to 1932 were published in 1935. The first was written by Don Lescohier and Elizabeth Brandeis and the second by Selig Perlman and Philip Taft.
2. Throughout this review, we draw heavily on Gallaway and Vedder 1996.
3. Economic growth was measured by the percentage growth in real personal income per capita. Union membership was measured by taking the average of the percentage of the labor force in labor unions early in the period (1983) and late in the period (1994). The high-union-membership states were New York, Hawaii, Michigan, Washington, New Jersey, Pennsylvania, Alaska, Oregon, Ohio, and Illinois. The low-union-membership states were South Carolina, North Carolina, Mississippi, Texas, Arkansas, Arizona, Florida, Georgia, Virginia, and South Dakota. The data are from the U. S. Department of Labor (union membership) and the U. S. Department of Commerce (income).
4. Union membership data are for 1994. The high-union states, so defined, differ slightly from those in n3, which averages data for 1983 and 1984. The high-union states are New York, Hawaii, Michigan, Washington, New Jersey, Alaska, Illinois, Minnesota, Pennsylvania, and Indiana; the low-membership states are the two Carolinas, Mississippi, Texas, Florida, Virginia, Arizona, Arkansas, South Dakota, and Idaho. Migration data are from the U. S. Department of Commerce, Bureau of the Census.
5. 295 U.S. 49; 79 L. Ed. 1570; 55 Sup. Ct. 837.1935.

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J. Bradford De Long

IT DOESN'T WORK

ABSTRACT: *Vedder and Gallaway are mistaken in their attempted demonstration that government policies to raise real wages have been the source of most or all U.S. unemployment in the twentieth century. Their case depends on a presumed correlation between high unemployment and high real wages that has not existed since World War II, and on a naive confusion between correlation and causation: just because real wages and unemployment were both relatively high during the Great Depression does not mean that high real wages were the cause of high unemployment. More likely, the reverse is true. The ascription of high unemployment to higher-than-equilibrium real wages is not always in error. But there is no reason whatsoever to believe Vedder and Gallaway's claim that the government's policies to raise wages have been "the major cause of high unemployment in the United States."*

There are two things wrong with Richard Vedder and Lowell Gallaway's *Out of Work: Unemployment and Government in Twentieth-Century America* (New York: Holmes and Meier, 1993). The first is that it confuses correlation with causation. It jumps from its authors' observation of high unemployment correlated with high real wages—the "real" purchasing power of what workers earn—to the conclusion

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J. Bradford De Long, Associate Professor of Economics, University of California at Berkeley, Berkeley, CA 94720, a research associate of the National Bureau of Economic Research and a visiting scholar at the Federal Reserve Bank of San Francisco, would like to thank the National Science Foundation and the Alfred P. Sloan Foundation for financial support, and Bob Barsky, Barry Eichengreen, Jeffrey Friedman, Chris Hanes, and Christie Romer for helpful comments and discussions.

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that any (government) action that increases real wages increases unemployment.

As the back jacket reports: "*Out of Work* shows . . . that such policies as minimum wages, legal privileges for unions, civil rights legislation, unemployment compensation, and welfare have all played significant roles in generating joblessness." The conclusion of the book, allegedly backed by "relentless and devastating evidence," is "that the major cause of high unemployment in the United States . . . is government itself."

But when we find high real wages and high unemployment occurring together, which is cause and which is effect? Is unemployment high because workers have relatively high purchasing power? Or are real wages high because unemployment is high? Economists have argued this back and forth for nearly the entire century, and the only sane answer is that sometimes it is one, sometimes it is the other, and sometimes some third factor is causing both.

In the United States in the 1930s, for example, the balance of the evidence is that real wages were high because unemployment was high—and that steps to reduce wage levels would have deepened, not alleviated, the Great Depression. In Europe in the late 1970s, on the other hand, the balance of the evidence is that unemployment was indeed high because real wages were higher than "warranted," as a result of the impact on relative prices of the 1973 tripling of world oil prices (see Bruno and Sachs 1983)—and steps to reduce wage levels probably would have reduced unemployment. Had the authors taken a more careful look at the direction of causation in their presumed correlation between high real wages and high unemployment, they would have been led to a much more measured conclusion than the thesis that any (government) action that increases real wages increases unemployment.

The first flaw in the book is bad, but the second is worse. There is no doubt that there are times, places, and historical episodes—the Great Depression in the United States or Europe in the late 1970s—in which high unemployment and high real wages do go together. But there is strong reason to doubt that this is a general pattern. When the business cycle turns downward, the jobs that disappear are disproportionately low-wage jobs held by the less skilled. The jobs that remain pay, on average, more. So the shifting cyclical composition of employment generates the appearance of a countercyclical

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pattern in real wages: the average real wage rises in recessions because more low-paid workers have lost their jobs.

When one corrects for this "composition of employment" effect, the balance of the evidence is that in the average business cycle real wages are weakly *procyclical:* a particular job with particular skill requirements tends to carry higher real wages in a boom than in a recession (see Barsky and Solon 1993). Real wages are high in booms because demand for investment goods is high in booms—and thus the derived demand for labor is high as well.

Moreover, Vedder and Gallaway's analysis handles long-run productivity growth incorrectly. Their theory is not that unemployment is high when real wages are high in absolute value, but when real wages are high relative to the trend of productivity growth. But the productivity series that they use to scale their real wage measure is wrong: it contains cyclical movements in productivity that are the result, not the cause, of low production and high unemployment.

So not only do Vedder and Gallaway confuse correlation with causation in those historical episodes in which high unemployment and high real wages go together, but their presumed general correlation of high wages and high unemployment is—most likely—an artifact of the way their data were constructed. High real wages (relative to productivity) cannot possibly be part of a general explanation for high unemployment, because real wages are more often than not *low* when unemployment is high.

Thus, there is virtually nothing left of Vedder and Gallaway's grand attempt to "redefine the way we think about one of the most explosive issues of the twentieth century." A lot of hard work has led them to false conclusions because it was based on flawed foundations. I do not pretend that I have the key to the riddle of the business cycle. But I do not think that Vedder and Gallaway have it, either.

Let me expand on why I have reached my negative judgment about this book. I will first discuss the pattern of real wages over the business cycle, and explain why I believe that the central correlation on which Vedder and Gallaway have based their argument is in all likelihood an artifact of how our data are constructed, not how the world really works. I will then go on to explain in more detail how it is that when high real wages and high unemployment go together, sometimes one is the cause and sometimes the other is, but the chain of causation is not clear and direct enough to justify laying responsi-

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bility for unemployment at the door of government policies to raise wages.

Real Wages Are Not Countercyclical

In 1936, John Maynard Keynes's *General Theory of Employment, Interest and Money* inaugurated the intellectual tradition of modern macroeconomics. One feature of the business cycle Keynes described—a feature he went to significant pains to analyze—was a presumed countercyclical nature of real wage movements: when production was high relative to trend, Keynes argued, unemployment and real wages were low; when production was low relative to trend—when the economy was in recession or depression—both unemployment and real wages were high.

This part of Keynes's book drew sharp and immediate dissent from economists specializing in the study of the labor market and labor institutions. Forty pages of the March 1939 *Economic Journal* were devoted to the question of cyclical movements in real wages. John Dunlop—then a young economist; later, chairman of the War Labor Board during World War II and Secretary of Labor under President Ford—contributed a substantial article, arguing that real wages were *not* countercyclical: that high unemployment and high real wages relative to trend did not go together. Lori Tarshis contributed a short comment to the same effect.

Keynes wrote a substantial reply, essentially conceding the point: Keynes said that he had tried to minimize points of difference between his approach and the previous "classical" approach; that countercyclical real wages had been an important part of the "classical" approach; that nothing important to his argument hinged on the countercyclical nature of real wages; and that he deferred to the superior statistical, institutional, and historical expertise of the labor economists.

Since then, economists have often revisited the question of the cyclical behavior of wages and prices. Recently, valuable contributions have come from Mark Bils (1985), Michael Keane, Robert Moffitt, and David Runkle (1988), and Paul Beaudry and John DiNardo (1991) in the *Journal of Political Economy;* Gary Solon, Robert Barsky, and Jonathan Parker (1994) in the *Quarterly Journal of Economics;* and

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Michael Bruno and Jeffrey Sachs (1982) in their book, *The Economics of Stagflation*.

For the most part, subsequent revisitations of this question have backed Dunlop: the real wage is weakly procyclical, with higher wages associated with low unemployment; or it is, at most, acyclical.

Now come Vedder and Gallaway to claim not only that 60 years of economic research has been wrong—that real wages are countercyclical, as Keynes believed, such that high unemployment is associated with high real wages—but that the association between high unemployment and high real wages is so strong as to account for practically all fluctuations in American unemployment over the past century.

Why this massive dissonance between the consensus of contributors to the University of Chicago's *Journal of Political Economy* and what is reported by Vedder and Gallaway? This is the question that first interested me in the book. And I quickly found that Vedder and Gallaway would be of little help in trying to reconcile these two lines of thought: neither the Bruno and Sachs study of Europe in the 1970s nor the *Journal of Political Economy* articles that contain the latest word on real wages make it into Vedder and Gallaway's bibliography.

One difficulty plaguing the topic is "composition bias" in measurements of economy-wide real wages. The jobs that vanish in depressions are disproportionately low-wage jobs held by the relatively lesser-skilled. The average wage is calculated by taking total payroll and dividing it by total work hours. Thus, anything that shifts the relative proportions of high-wage and low-wage jobs will change the calculated average wage—and a recession or a depression shifts the relative proportions of high-wage and low-wage jobs.

Note that this "composition bias" is all effect and no cause: in its pure form the wages paid to any one individual doing any one job have not changed, so there has been nothing to alter employers' decisions to hire or retain workers: yet the economy-wide average real wage does change, because the average contains fewer low-wage workers as a result of recession- or depression-induced unemployment. Vedder and Gallaway, however, take no account of these complications.

A second difficulty is that Vedder and Gallaway have committed an error in how they scale their real-wage variable. Real wages are high or low relative to the productive potential of the average worker, so Vedder and Gallaway divide real wages by a measure of productivity. Unfortunately, they divide real wages not by *potential* productivity—

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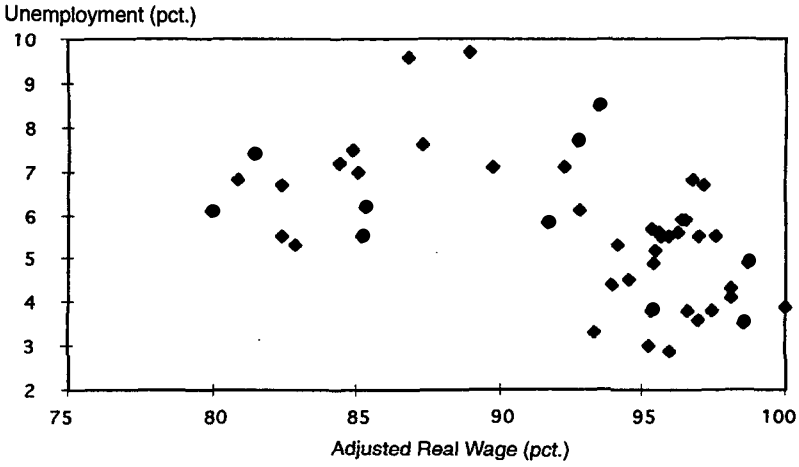
not by what output per hour would have been if factories had been running at normal levels of operation—but by *actual* productivity. And actual productivity varies greatly over the business cycle. Whenever production is depressed, measured productivity is relatively low because the American economy possesses substantial increasing returns to scale: the "overhead" workers in a factory are necessary to keep it from rusting into oblivion, whether the factory is running three, two, one, or zero shifts.

The "composition bias" and the "productivity bias" serve to associate Vedder and Gallaway's measure of real wages more strongly with high unemployment than a properly constructed measure would.

A third—and perhaps the most important—difficulty is that the American economy may well not have the same structure today that it had before World War II. During the Great Depression, real wages rose, while prices of commodities in the consumer price index fell faster than did the wages paid to workers. During the Great Depression there was a positive correlation between high real wages and high unemployment.

Since World War II there has been no such correlation. Figure 1 plots my calculations of productivity-adjusted real wages and the unemployment rate. Those postwar years when unemployment was the highest were not the years in which adjusted real wages were the

Figure 1. Postwar productivity-adjusted real wages and unemployment



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highest. And the postwar years, when unemployment was the lowest, were not the years in which adjusted real wages were the lowest.

Thus, Vedder and Gallaway's book is fatally flawed. The presumed correlation between high adjusted real wages and high unemployment is the foundation on which they build their argument. This foundation is made out of mud. Since World War II, there is no sign that high adjusted real wages go with high unemployment; real wages appear procyclical and not countercyclical. Before the Great Depression, real wages may have been countercyclical, but because no one has unscrambled the effects of "composition bias" and "productivity bias" on Vedder and Gallaway's adjusted real wage measure, we really do not know.

High Real Wages Sometimes Are the Result of Unemployment

During the Great Depression, real wages were certainly countercyclical: prices of commodities in the consumer price index fell faster than did the wages paid to workers. There was therefore a positive correlation between high real wages and high unemployment. But there is no good reason to think that high real wages in the Depression were a cause, rather than an effect, of high unemployment.

When Keynes wrote his *General Theory* (1936), he was convinced (i) that real wages were higher in recessions than in booms, and (ii) that the cause of the business cycle lay in fluctuations in aggregate demand: fluctuations both in the marginal propensity of households to consume and in the "animal spirits" of investors, whose actions determined the demand for capital goods. In Keynes's vision, there was correlation between high real wages and high unemployment, but no causation: if the government, workers, firms, or unions took steps to reduce nominal wages that had no impact on real aggregate demand, the likely consequence would not be a fall, but a rise, in unemployment.

How could this be? Keynes's vision of the economy was one in which the nominal wages paid to workers are relatively slow to respond to changing demand and supply conditions. In times of recession, firms find themselves able to hire additional workers from the unemployed at the prevailing wage. They have every incentive to do so. But to sell the output additional workers produce, firms have to

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cut their prices a little bit below those of their competitors in order to expand their market share.

When every firm cuts its price relative to the prevailing nominal wage, the level of prices falls relative to the prevailing nominal wage. This process of mutual price-cutting continues until the price level has fallen so far that the extra product of an additional worker to a representative firm can no longer be sold at a profit. At that price level (relative to the nominal wage), it doesn't pay firms to hire more workers. Thus, during a recession, real wages are relatively high because the price level is relatively low.

What did Keynes think would happen if nominal wages were *not* relatively inflexible in a recession? What if nominal wages fell sharply as unemployment rose? The first-order effect in Keynes's model is that a fall in nominal wages would have no effect on employment: with a lower wage level, firms would find it worthwhile to continue the downward spiral of price-cutting even further. The end result? An economy with the same real wage, the same level of unemployment, and lower nominal wages and prices. The second-order effect in Keynes's model is that a fall in nominal wages (and the induced further fall in product prices) *raises* unemployment: falling prices discourage investment because they generate (i) extremely high real interest rates and (ii) large-scale bankruptcies and financial collapses that destroy the web of financial intermediation. The third-order effect in Keynes's model is that a fall in nominal wages (and the induced further fall in product prices) increases the purchasing power of households' real money balances, stimulates consumption, raises aggregate demand, and boosts employment.

Which effect is dominant? De Long and Summers (1988, extending Tobin 1975) argue that in economies with a Keynesian structure and with parameter values like those of the United States today, the second effect dominates: increasing nominal wage flexibility would increase the volatility of the business cycle.

What do Vedder and Gallaway have to say in response to Keynes's extended argument that causation runs from unemployment to high real wages, and that policies to lower nominal wages in a depression would be unhelpful?

They have little to say.

On page 47 they assert that any belief that "higher unemployment causes higher adjusted real wages" is "implausible. . . . It makes sense that higher wages would price workers out of the market, causing in-

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creased unemployment, but it makes no sense that higher unemployment would cause wages to rise (if anything, higher unemployment might induce wage-cutting)." But this is to misunderstand Keynes and the subsequent literature. The argument was never that higher unemployment would cause (nominal) wages to *rise*. The argument was, and is, that excess inventories and deficient demand for goods would cause the prices of goods to *fall;* that in modern economies the prices of goods would fall farther and faster than nominal wages—all nominal wages and prices are falling in a depression, but at different speeds—and that the net result would be higher real wages, because nominal wage levels would be relatively slow to fall.

This argument Vedder and Gallaway do not acknowledge: they never even try to come to grips with their principal intellectual opponent. They have offered no reasons to accept their assumption that a high unemployment/high real wage correlation—when it exists—is the result of high real wages causing high unemployment. They have given no reasons to reject Keynes's theory that, when such a correlation exists, it is the result of deficient aggregate demand causing high unemployment and also pushing down product prices, leading to a relatively high real wage.

Their third sentence is that the "Keynesian Revolution led the economics profession down an unproductive, destructive path for decades." In saying this, Vedder and Gallaway assume the obligation to give reasons why the Keynesian approach is incorrect, unproductive, and destructive. Yet they do not; they make a contract with their readers, and then casually break it.

None of this means that high real wages are always the consequence and never the cause of high unemployment. Edmond Malinvaud (1977) advances a more complex—and much more reasonable—position in his attempt to extend the scope of Keynesian analysis. Malinvaud argues that the market economy is subject to many possible macroeconomic maladies, and that not all business cycles—not all booms, not all depressions—are alike. Malinvaud distinguishes between "classical" and "Keynesian" unemployment. In both cases, the real wage paid to employed workers is higher than in full-employment equilibrium. But in the second case, high real wages are the result of a recession, while in the first case they are the cause.

In the same vein, Michael Bruno and Jeffrey Sachs (1982) provide a powerful and detailed analysis attributing high European unemployment in the wake of the 1973 oil shock and productivity slowdown

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to real wages that exceeded "warranted" levels. The French economist Daniel Cohen (1996) believes that high European unemployment today exists because "the war on unemployment is in the hands of governments which represent first and foremost the point of view of the people who have jobs and fear losing them." Thus, unemployment has been "feared less than the remedies that would have been necessary to contain it."

In Cohen's view, European unemployment could have been reduced by freeing up the labor market, but then the wages of the currently employed would have fallen as "those who have jobs are forced to compete with those who do not." European unemployment could have been reduced if the government had been willing to boost public employment and spend large sums on reentry programs for the jobless, thus increasing the value of the work that the currently unemployed could do. But these costly reentry programs must be paid for by higher taxation, and the fear is either that higher taxation will slow economic growth, or that programs for the unemployed will crowd out other social benefits. The European Union has been stuck in the middle.

So the ascription of high unemployment to higher-than-equilibrium real wages is not always wrong. There are times and places when it is right.

But Vedder and Gallaway are mistaken when they attempt to demonstrate that government policies to raise real wages have been *the* source of most, or all, U.S. unemployment in the twentieth century. They are mistaken because at the foundations of their argument are a presumed correlation between high unemployment and high real wages that does not exist today, and a confusion about how to distinguish cause from effect in macroeconomic analysis.

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EX9

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Warren J. Samuels

MURRAY ROTHBARD'S AUSTRIAN PERSPECTIVE ON THE HISTORY OF ECONOMIC THOUGHT

ABSTRACT: *Murray Rothbard's* Austrian Perspective on the History of Economic Thought *demonstrates his mastery of the literature. But his interpretation of the development of economics reflects, and is therefore severely limited by, his Austrian-libertarian perspective. Indeed, Rothbard appropriates the history of economic thought principally to advance his perspective, as seen in his neglect of social control, his identification of his desired economic system with the natural order of things, and especially in his denigratory treatment of Adam Smith—at bottom for not being an Austrian economist and a true libertarian. A partly informed, partly myopic and sometimes useful interpretation, this is the work of an ideologue.*

The late Murray Rothbard is well known for his versions of libertarianism and Austrian economics and for his love of controversy.[1] He is less well known, perhaps, for his considerable erudition in the field of the history of economic thought. His *Economic Thought before Adam Smith* and *Classical Economics* (Brookfield, Vt.: Edward Elgar, 1995), constituting volumes I and II of "An Austrian Perspective on the History of Economic Thought," demonstrate his mastery of both the

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Warren J. Samuels, Professor Emeritus, Department of Economics, Michigan State University, East Lansing, MI 48824, e-mail samuels@pilot.msu.edu, is coauthor of *The Economy as a Process of Valuation* (Elgar, 1997) and *Economic Thought and Discourse in the Twentieth Century* (Elgar, 1993).

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original and the interpretive literature pertaining to those periods. Had he concentrated on doing history of economic thought, he could possibly have become a leading authority in the field. Because of his private agenda and perspective (at least the latter of which would not have rendered him unique), he certainly would have been a heterodox and controversial historian of thought, but a well-read one, someone to reckon with. But given that the history of economic thought does not write itself and is an essentially interpretive enterprise, and given his own description of these volumes as delivering "An Austrian Perspective," the question inevitably arises whether Rothbard appropriates the history of economics to advance Austrian economics.

Rothbard's Libertarianism and His Austrianism

Rothbard attempts to find precursors to the Austrian program in general and the Misesian one in particular in order to establish that long before Adam Smith, Scholastic writers had accepted both competitive market prices and a subjective utility/scarcity theory of value; indeed, the dominant thought in France and Italy in the seventeenth and especially eighteenth centuries was "proto-Austrian" along these lines. The matter of the Scholastic just price, however, is more complicated than this. It is more than likely, for example, that market prices (competitive or otherwise) were invoked in canonical courts and arbitration situations *against* putatively extortionary prices actually found in the real world. Many Scholastic writers had multifaceted conceptions of what the just price was all about, comprising intrinsic value, social need, and other considerations. The complete story of Scholastic price/value theory changed over time and was perhaps more complicated than Rothbard makes it out to be. Here he lets his Austrian orientation get the better of him. And he fails to emphasize the gravamen of Scholasticism, namely, the superimposition of a theologically grounded system of ethics on the whole of life, including economic activity. Scholasticism was an intellectual derivative of religion as social control. In ignoring this, Rothbard lets his libertarianism direct his interpretive judgment.

As for Smith, he "actually took the sound, and almost fully developed, proto-Austrian subjective value tradition, and tragically shunted economics on to a false path, a dead end from which the Austrians

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had to rescue economics a century later" (1995a, xi). The language is an echo of William Stanley Jevons's dismissal of the value theory of David Ricardo; also echoing Jevons on the role of authority, Rothbard refers to "the tyranny of the British classical model, re-established by Mill in 1848" (1995b, 471). Indeed, he commences his chapter on "The celebrated Adam Smith" as follows:

> Adam Smith (1723-90) is a mystery in a puzzle wrapped in an enigma. The mystery is the enormous and unprecedented gap between Smith's exalted reputation and the reality of his dubious contribution to economic thought. (1995a, 435)

Rothbard's main complaint about Smith is his adoption of the labor theory of value. This eventually became transformed, in the hands of others, into a real-cost theory of value; and with Alfred Marshall, costs became the dominant factors in long-run price, with utility (taste, preference) dominant in the short run. Yet Rothbard's Austrian approach, for all its admirable emphasis on subjectivism and choice, is no conclusive basis on which to condemn Smith's confused theory of value; the confusions might have been resolved in a non-Austrian direction.

Another complaint targets Smith's ostensibly legendary laissez faire. Rothbard criticizes Smith for treating laissez faire as only a qualified presumption and for including a long list of exceptions to it in *The Wealth of Nations*, thereby departing from the ontology of a harmonious natural order—"in which government interference can only cripple and distort"—said to be evident in the *Theory of Moral Sentiments* (465 and passim). But to suggest that those who exalt Smith's reputation have ignored any of this is mistaken. Lionel Robbins (1952) emphasized the important economic role government played for Smith and the English classical economists, and he pointed out that they did not believe in an automatic, perfect harmony of interests. Other writers, including this one (Samuels 1966), have emphasized the critical role, for Smith et al., of government in the construction of a market/capitalist economy increasingly dominated by nonlanded property, and perforce of government as an instrument in modifying and perfecting of such an economy. For Smith in particular, the economy had its foundations not only in law and jurisprudence but in the exercise of the moral sentiments that stand behind moral rules. The idea of laissez faire for Smith (who did not use the

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term) was severely circumscribed, and was directed largely against mercantilism.

In objecting to Smith's failure to be a consistent noninterventionist, Rothbard's Austrian libertarianism not only engenders myopia and wishful thinking but involves the selective reification and absolutist legitimation of economic institutions that have not only changed over time but emerged in no small degree because of government action. This is unsurprising, to all but the myopic libertarian, because the institutions that form the economy are *legal* (and thus always coercive) in character—or, as I would prefer to put it, they emerge and change on the basis of the legal-economic nexus (Samuels 1989). If Smith knew about anything, it was the difficulty involved in working out the moral and legal rules necessary for organized living, the division of labor, and the production of the wealth of nations. All this is finessed and obfuscated by Rothbard's naive libertarianism.

Rothbard devotes 30 percent of his second volume to Marx, but for all the deficiencies of Marx's analysis, Rothbard's critique is unsatisfactory. Take, for example, his treatment of "class." For Rothbard, "all classes live in harmony through the voluntary exchange of goods and services that mutually benefits them all"; capitalists and workers have no antagonistic class interests (1995b, 380, 382). Rothbard thus trivializes the idea of class, making it disappear—except when it is supported by state power. One wonders how Rothbard would have treated Vilfredo Pareto's analyses of class. That believer in utility theory had no libertarian illusions about power and politics. Rothbard treats bourgeois society as if its structure were derivative of the natural order of things, rather than being a product of centuries of middle-class capture and use of government power. The fact that *past* uses of power have been rendered invisible by their codification in property law does not render them parts of a harmonious, natural universe.

Rothbard concludes his treatment of Marx with the argument that "the well-known horrors of twentieth-century communism . . . of Lenin, Stalin, Mao and Pol Pot, can be considered the logical unfolding, the embodiment, of the nineteenth-century vision of their master, Karl Marx" (1995b, 433). Does it make more sense to say this than to blame Jesus or Mohammed for the excesses of those religionists who have pretended to speak in their names, or the excesses of various businessmen to Adam Smith, Ludwig von Mises, or Murray Rothbard?

For all of Rothbard's erudition, the fact is that his account of the

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history of economic thought is driven by, and attempts to reinforce, a particular view of the world. That view, in my opinion, is often laudable in its sentiments but simplistic, flawed, and subject to abuse on behalf of the economic oligarchy that lurks behind Rothbard's images of wondrous harmony and beneficence. Pervading Rothbard's text is (of course) the assumption of the Truth of his story. For believers in libertarianism and/or Austrian economics (or Rothbard's versions thereof), the assumption may seem warranted. Rothbard's single-minded dedication to his particular perspective serves as a useful heuristic tool. The correlative problem is, however, that there is ample reason to view him as an ideologue and, as a result, to discount his work's many interesting insights into the history of economic thought.

NOTES

1. The importance of Rothbard within contemporary Austrian economics is evidenced by the prominent discussions of his work in Boettke 1994 (see p. 625) and Vaughn 1994 (see especially, but not solely, pp. 99-100). Within Austrian economics Rothbard was a follower of Ludwig von Mises (rather than, for example, of Friedrich von Hayek).

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Werner Troesken

POSTWAR ECONOMIC STABILIZATION: A STYLIZED HALF-TRUTH

ABSTRACT: *Conventional wisdom maintains that without government intervention, capitalism is prone to collapse, as it did during the 1930s, and that only Keynesian policies have stabilized post-World War II capitalism. But recent research suggests that postwar economic stabilization is largely a statistical artifact, the result of poor prewar data, and that the Great Depression was caused, not by the inherent instability of capitalism, but by policy errors made by government agencies. Thus, we should not be so quick to credit the economic successes of the last 70 years to enlightened intervention.*

There is a well-known story that goes like this. Before World War II, the U.S. economy was highly unstable, forever cycling between boom and bust. To see this, look no further than the Great Depression: a staggering economic boom during the 1920s was followed by an even more staggering economic collapse during the 1930s. After World War II, the economy stabilized with the ascendancy of Keynesian economics and a more activist government. Many conclude from this story that activist fiscal and monetary policies and government regulation of financial markets are desirable, mitigating the vagaries of unfettered capitalism. Without such policies, they say, the free market is prone to collapse.

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Werner Troesken, Associate Professor of History and Economics, Department of History, University of Pittsburgh, Pittsburgh, PA 15260-7403, e-mail troesken+@pitt.edu, thanks Dave DeJong, Jeffrey Friedman, John Murray, and Rick Tannery for helpful comments and discussions. The usual disclaimer applies.

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Economists refer to the stabilization of the postwar economy as a "stylized fact." Stylized facts are compact and simplified abstractions of larger, more complex realities. As such, they are intended to communicate basic truths without the distraction of unimportant details and messy caveats. Of course, by eliminating the details and caveats, stylized facts communicate little lies along with larger truths. Unfortunately, so many details and caveats are sometimes omitted that the remaining abstractions are closer to stylized half-truths than stylized facts.

The story about postwar stabilization with which I began is more a stylized half-truth than a stylized fact. Two parts of the story are accurate, however. The postwar economy is more stable than the prewar economy, and part of this stabilization can undoubtedly be attributed to government policy. But three other things need to be said.

First, the Great Depression was caused by a government-engineered monetary contraction and by a wave of bank panics that would not have occurred, or would have been much less severe, without ill-advised government policies. The lesson that emerges from a correct understanding of the Great Depression is not that activist government is needed to stabilize the economy. To the contrary, activist government can be a source of instability. Indeed, it was government policy that turned a moderate economic downturn into the worst depression Americans have ever seen.

Second, the usual story probably overstates the degree of economic stabilization that has occurred in the postwar era. Christina Romer has shown that postwar economic stabilization is largely a statistical artifact, the result of poor prewar data. Once the data are corrected, the prewar economy appears to be about half as volatile as it otherwise would, and only slightly more volatile than the postwar economy. Although later research has challenged Romer's findings, her work raises serious questions about the robustness of the data used to demonstrate the increased stability of the postwar economy.

Third, postwar government stabilization policies have hardly been a complete success. A wide range of policies has proved destabilizing. For example, deposit insurance and the Glass-Steagall Act—policies ostensibly designed to make the economy more stable—have undermined the stability and health of the banking industry. There is even evidence that antitrust regulation, which one might think irrelevant to economic stability, has reduced real economic output and generated financial panics.

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Romer's Revised Data on the Pre- and Postwar Economies

When researchers want to compare the stability of the post-World War II economy with that of the prewar economy, they typically reach for the *Historical Statistics of the United States*. This source allows researchers to compare the volatility of GNP and unemployment rates before and after World War II. Such comparisons invariably show that GNP and unemployment are more stable after the war than before. For many, this pattern establishes a correlation between government activism and economic stability. It seems that when the government does not pursue stabilization policies, as in the prewar era, the economy has been highly volatile. In contrast, when the government pursues stabilization policies, as it has since World War II, the economy has been much less volatile. This reasoning, however, depends critically on the nature of the GNP and unemployment data found in the *Historical Statistics* and other widely used sources. Since the prewar data are constructed using different methods than the postwar data, one might question how appropriate it is to use these data series to reach conclusions about comparative economic volatility.

Consider first estimates of GNP. The prewar GNP estimates found in the *Historical Statistics* are built on the pioneering work of Simon Kuznets, who relied primarily on data about commodity output. He assumed that noncommodity output, such as transportation and services, moved in proportion with commodity output. While this procedure portrays long-term trends in economic growth accurately, it exaggerates the magnitude of cyclical fluctuations because, as suggested by both economic theory and modern experience, the noncommodity components of GNP are less volatile than the commodity components. Transportation and services, contrary to Kuznets's assumption, move less than commodity output (Romer 1989).

This presents a problem. Ideally, one would like to go back in time and construct an actual data series on noncommodity output. With these data one could then estimate the exact relationship between commodity and noncommodity output. But this is not possible. In lieu of estimating the relationship between commodity and noncommodity output using the actual historical data, Romer (1989) uses data from the interwar and postwar periods—periods for which the necessary data are available—to identify the relationship between commodity and noncommodity output during the late nineteenth

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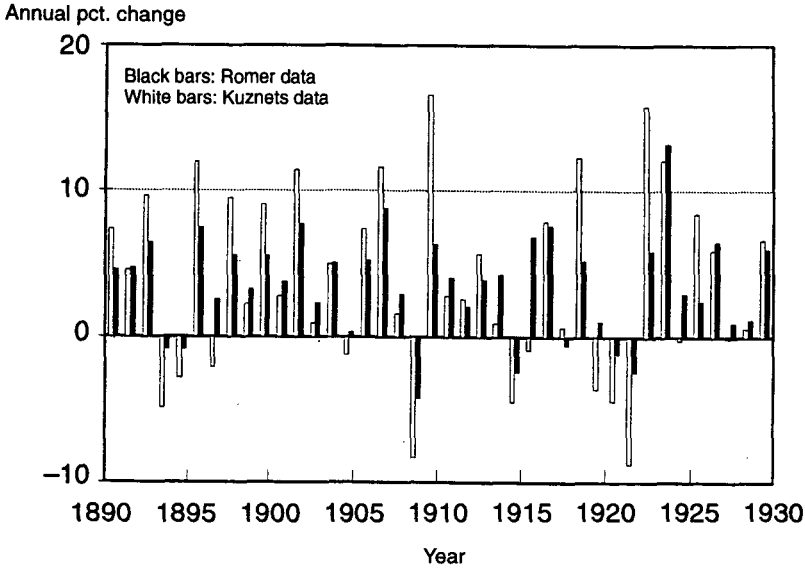
Figure 1. Change in GNP, 1890–1930



Sources: Romer 1989; *Historical Statistics of the United States* (1976), 228, Series F32.

and early twentieth century. Assuming that the relationship between noncommodity output and commodity output was the same earlier that it was in the interwar and postwar periods, Romer has argued that the prewar economy was about 30 percent more volatile than the postwar economy, about half as much as previously estimated. Figure 1 plots the annual percentage change in real GNP using the Romer and Kuznets data series and illustrates the effects of correcting the data. In terms of absolute value, the annual percentage changes in the Romer series are much smaller than the annual percentage changes in the Kuznets (*Historical Statistics*) series.

Similarly, for the years after 1940, the unemployment rate series is based on the Current Population Survey. For the years before 1940, unemployment rates are estimates constructed by Stanley Lebergott. To construct these estimates, Lebergott culled information from the decennial census, industry records, and state reports. The census data provided Lebergott with benchmarks for ten-year intervals. He then used a complicated interpolation procedure to estimate unemployment rates for the intervening years (Romer 1986).

Like early estimates of GNP, Lebergott's estimates of unemploy-

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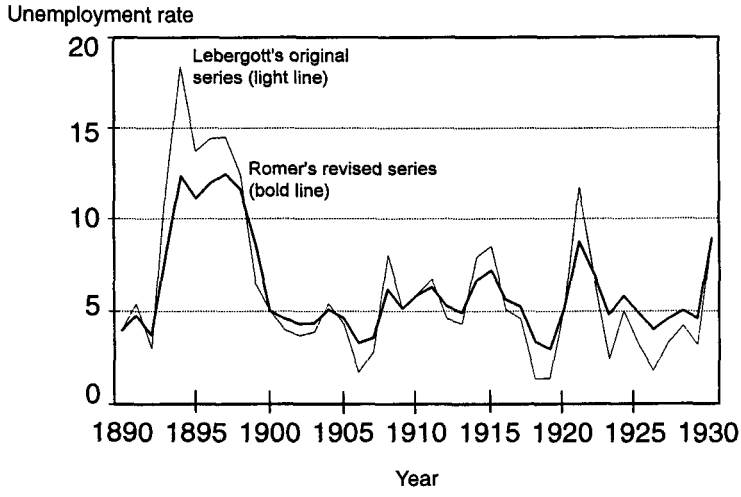
ment accurately portray long-run trends but overstate the variability of short-run cycles. In particular, they do not reflect the movement of discouraged workers into and out of the labor force. During boom periods, discouraged workers—workers who are not actively seeking employment, and are therefore not counted as part of the labor force—return to the labor force and begin looking for employment. This should increase the unemployment rate. During recessions, many of these workers get disheartened and leave the labor force. This should reduce the unemployment rate. Because he ignores the countercyclical movements of discouraged workers, Lebergott overstates unemployment during recessions and understates unemployment during expansions (Romer 1986).

The ideal solution to this problem would be to get actual data on the behavior of discouraged workers during the prewar era. But again this is not possible, so Romer follows the same basic procedure to reestimate prewar unemployment rates that she used to revise prewar GNP. She uses information about the movement of discouraged workers during the postwar era to estimate movements during the prewar era. Romer's revised estimates are much less volatile than Lebergott's. This can be seen in Figure 2, which plots both series.

Figure 2. Unemployment Rates, 1890–1930



Sources: Romer 1989; *Historical Statistics of the United States* (1976), 135, Series D86.

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The uncorrected data suggest, for example, that the depression of the 1890s rivaled the Great Depression. The corrected data show that the earlier downturn was no worse than the Reagan recession of 1982.

Romer's work has generated a vast literature—the *Social Science Citation Index* lists over 250 references to her research. Among the most prominent critiques and reconsiderations of Romer's work is that of Nathan Balke and Robert Gordon (1989), who use different procedures to estimate prewar GNP and conclude that prewar GNP was nearly twice as volatile as postwar GNP. David Weir (1986) suggests that Romer's revised estimates can be undone by minor changes in estimating procedures. Weir's own estimates of unemployment before 1900 are actually lower than Romer's, but are higher and more volatile than Romer's estimates for the 1900–1930 period (Weir 1992). And Lebergott (1986; also see idem, 1992) argues, among other things, that it is not appropriate to impose postwar relationships on the prewar economy.

Exploring the length of economic recessions and expansions in the pre- and postwar eras, however, Mark Watson (1994) finds evidence that supports Romer. Other researchers have claimed that during the postwar era, recessions have become shorter while expansions have become longer (Diebold and Rudebusch 1992). This, they claim, attests to the efficacy of stabilization policies in the postwar era. Watson (1994), however, shows that postwar recessions merely appear shorter, and expansions longer, because of poor prewar data. The net result of the controversy is that Romer's estimates substantially change the standard story about postwar economic stabilization. The postwar economy now appears, at best, only slightly more stable than the prewar economy (Romer 1986).

Reconsidering the Great Depression

Campaigning for president in the summer of 1928, Herbert Hoover alluded to the Republican policies of the previous eight years and proclaimed:

> We in America today are nearer to the final triumph over poverty than ever before in the history of any land. The poorhouse is vanishing from among us. We have not yet reached our goal, but, given the chance to go forward with the policies of the last eight years, we shall

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> soon, with the help of God, be in sight of the day when poverty will be banished from this nation.

History has not been kind to Hoover and his optimism. A few years after he said these words, America was in the midst of the worst economic downturn it had ever seen and people were living in shantytowns derisively called Hoovervilles. Yet in 1928 Americans had good reason to be optimistic about the economy. Between 1920 and 1928, the unemployment rate averaged between 2 and 3 percent; earnings for nonfarm employees rose by 30 percent; and the average share of stock more than doubled in value. No wonder 58 percent the electorate cast its ballots for Hoover in the 1928 election, a large margin of victory by historical standards.[1] Weeks before the stock market crash, America's most prominent economist, Irving Fisher, notoriously declared that "stock prices have reached what looks like a permanently high plateau. . . . There might be a recession in stock prices but nothing in the nature of a crash" (Walton and Rockoff 1994, 512).

The current view of postwar economic stabilization is reminiscent of Hoover's and Fisher's optimism. Both our gloating and theirs imply that contemporary policy makers are wiser than those of the past. And there is some warrant for such complacency. The economy did in fact grow rapidly during the 1920s, and the policies of Hoover's fellow Republicans, Harding and Coolidge, may well have contributed to that growth. Similarly, the postwar economy is more stable than the prewar economy, and much of this stability probably stems from the fact that policy makers and economists have learned from the mistakes of the past. For example, in response to the stock market crash of 1987, the Fed avoided making the mistakes it had made during the Great Depression.[2] But the unbridled optimism of the 1920s blinded politicians and prominent economists to the conditions that ultimately generated the Great Depression. Gloating about how much smarter economists and policy makers are today than they were in the past can have the same blinding effect.

Alan Brinkley (1997, 682–83) identifies five factors as contributing to the Depression:

> One factor . . . was a lack of diversification in the American economy in the 1920s. Prosperity had depended excessively on a few basic industries, notably construction and automobiles. . . . A second important factor was the maldistribution of purchasing power and, as a re-

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> sult, a weakness in consumer demand. . . . Demand was not keeping up with supply. A third major problem was the credit structure of the economy. Farmers were deeply debt . . . . A fourth factor was America's position in international trade. Late in the 1920s, European demand for American goods began to decline . . . . The international debt structure . . . was a fifth contributing factor to the Great Depression . . . . The collapse of the international credit structure was one of the reasons the Depression spread to Europe (and grew much worse in America) after 1931.

Although many historians share Brinkley's views, few economists do. Brinkley argues that the economy was not sufficiently diversified in 1929, yet it was producing a much greater variety of goods than it had, say, in 1840, when it was based mostly on agricultural production. If diversification were important, then the economy of the early 1800s should have exhibited downturns as severe as the Great Depression. It did not.

As for income inequality, if this contributed to the Great Depression, it seems that economic downturns should generally follow periods of increasing inequality. It is far from clear, however, that such a correlation exists. Moreover, although income inequality did rise during the 1920s, economists have shown that the economic boom of the 1920s benefitted a broad cross-section of the American population (Lebergott 1962, 248–99; Smiley 1983). As mentioned above, income for the average nonfarm worker rose by nearly a third during the 1920s.

Brinkley's third cause—the nation's credit structure—contains a kernel of truth. The collapse of the American banking system did play a role in the Great Depression. This role is considered below. Brinkley's forth and fifth causes, which concern international forces, also contain some truth. Recent research suggests that trade restrictions and exchange rate policies might have helped push the United States into a downturn (Eichengreen 1989; Temin 1989). But changes in international trade accounted for only a tiny portion of changes in GNP during the Great Depression, suggesting that the role of international trade was minor (Romer 1993).

Most economists believe that the Great Depression started off as a moderate economic downturn after the stock market crash prompted a drop in consumption, especially the consumption of durable goods such as cars and large household appliances (Romer 1990). The crash

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caused people to cut back on spending for two reasons. First, it reduced wealth, leading people to buy less. Second, to the degree that people used the stock market as a barometer of future economic activity, the crash raised concerns about an economic slowdown and encouraged people to put off buying big-ticket items. The economy, however, would have quickly recovered from the crash-induced slowdown had the Federal Reserve not pursued policies that led to the collapse of the nation's monetary and banking system.

The Fed sinned twice, once when it engineered a huge reduction in the money supply, and again when it contributed to the banking crisis. Between 1929 and 1932, the Fed reduced the nation's real money stock by a third, by far the largest monetary contraction in American history (Friedman and Schwartz 1963, 299). By driving up real interest rates, this massive reduction in the money supply made it more expensive for businesses to borrow money to maintain and expand their capital stocks (Romer 1993). At the peak of the Depression, the nation's net capital stock was shrinking; because it was so costly to service existing and future debt, business owners could not afford to buy new equipment or even maintain their existing stock. They had to let their factories and machines fall into disrepair. In the face of nominal wage rigidities, the monetary contraction also caused real wages to rise above their market-clearing levels. Widespread unemployment followed (O'Brien 1990).

When Milton Friedman and Anna Schwartz first argued that monetary forces caused the Great Depression, their thesis was greeted with much skepticism. Thirty years later, though, most economists have been persuaded that the Federal Reserve played a fundamental role in propagating the Depression.[3] Three pieces of evidence have proven especially compelling. First, countries that clung to the gold standard and pursued tight monetary policies, such as the United States, experienced severe downturns, while countries that abandoned the gold standard and pursued loose monetary policies, like Great Britain and Japan, experienced much more mild downturns (Bernanke 1995; Temin 1989). Second, Friedman and Schwartz (1963) establish an historical correlation between monetary shocks and economic output; throughout American history, periods of monetary contraction have been associated with economic slumps. Figure 3, which plots the average annual change in M2 (one measure of the money supply), highlights this correlation. Third, just as a massive monetary contraction ushered in the Depression, monetary expan-

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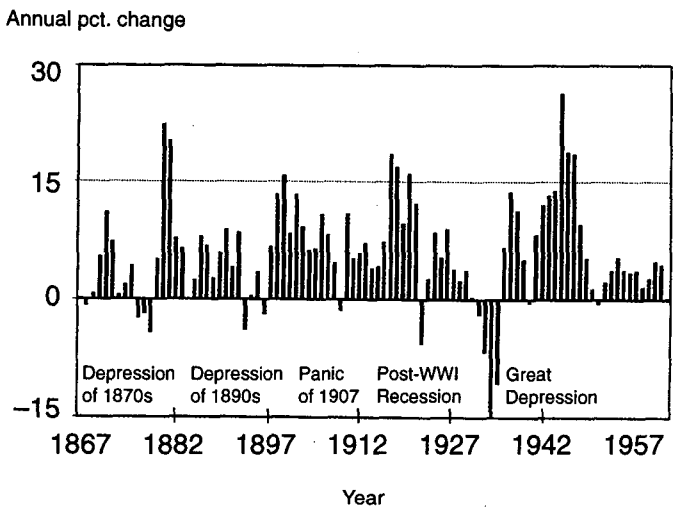
Figure 3. Change in Money Supply (M2), 1867–1960



Sources: *Historical Statistics of the United States* (1976), 992–93, Series X415.

sions brought about recoveries in the United States and elsewhere (Bernanke 1995; Romer 1992; Temin 1989).

Why did the Fed contract the money supply in the midst of an economic downturn? Friedman and Schwartz argue that a critical event was the death in 1928 of Benjamin Strong, governor of the Federal Reserve Bank of New York. Strong's personal correspondence, as well as comments by Irving Fisher and various members of the Fed, suggest that had he survived, he would have known how to address the liquidity crisis that caused the Depression (Friedman and Schwartz 1963, 412–14). Peter Temin takes issue with this view, arguing that Strong's death was a "minor event in the history of the Great Depression." Temin believes instead that rigid adherence to the gold standard drove policy makers to pursue deflationary policies (Temin 1989, 34–35). More recently, David Wheelock has attributed the Fed's poor policy decisions to the method it used to monitor the money supply during the 1920s and 30s. As Wheelock explains, the Fed measured monetary tightness or ease by looking at the borrowing habits of banks that were members of the Federal Reserve System: "heavy borrowing indicated tight money and little borrowing indi-

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cated monetary ease." This policy "resulted in procyclical changes in the supply of money" and "permitted greater declines in the money supply during severe recessions than during minor ones" (Wheelock 1991, 114–15).

The Fed's other big mistake was how it responded to the collapse of the nation's banking system. At the peak of the Great Depression, thousands of banks were failing every year. In 1933 alone, roughly 4,000 banks failed. Concerned that their bank would be the next to fail, depositors grew nervous and withdrew their money from the system. Money that had been used to make loans to businesses and homeowners now sat idle in shoeboxes or stuffed under mattresses. Moreover, like the stock-market crash, bank panics eroded business and consumer confidence, further slowing business investment and spending on consumer durables (Bernanke 1983; Telser 1996).

The Fed could have prevented many bank failures and mitigated the severity of the bank panics by acting as a lender of last resort. However, believing that bank failures were caused by poor management and loan decisions, the Fed usually chose not to bail out failing banks (Friedman and Schwartz 1963, 357–59). During the nineteenth century, interbank clearing houses—private institutions created by banks to facilitate check clearing—had often acted as lenders of last resort in times of panic (Gorton and Mullineaux 1987). This prevented isolated bank runs from having the contagion effects they had during the Great Depression. If the Fed had done the same during the 1930s, the crisis in the nation's banking system would not have been as severe. Beyond this, George Selgin (1994) has argued that bank failures throughout the 1920s and 1930s stemmed, in large part, from government restrictions that prevented small banks in agricultural regions from diversifying by opening branches in other regions, which would have diminished the risks unique to agriculture.

Another possible culprit, ironically, was the Reconstruction Finance Corporation (RFC), which was designed to prevent bank panics and failures. Established in January of 1932, the RFC made low-interest loans to failing banks and railroads. Friedman and Schwartz (1963, 320–21 and 330–31) suggest that net effect of the RFC was minimal. James Butkiewicz has presented econometric evidence that RFC loans reduced the number of bank failures (Butkiewicz 1995). Lester Telser, however, argues that the bumblings of the RFC contributed to bank panics in Illinois and Michigan (Telser 1996).

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In the midst of the Great Depression, Rexford Tugwell, a key advisor to President Roosevelt, proclaimed:

> The cat is out of the bag. There is no invisible hand. There never was. If the depression has not taught us that, we are incapable of education. . . . We must now supply a real and visible guiding hand to do the task which that mythical, nonexistent, invisible agency was supposed to perform, but never did. (In Walton and Rockoff 1994, 53.)

Tugwell's view is now widely shared, despite the alleged triumph of laissez-faire ideas. Without government policies, it is thought, the market is prone to collapse. This is the wrong lesson to draw from the Great Depression. If anything, this disastrous episode illustrates what can happen when government agencies have too much regulatory discretion. Bennett McCallum has shown that if the Federal Reserve had followed a simple monetary base rule—the type of rule long advocated by such economists as Milton Friedman—the Great Depression would have been much less severe (McCallum 1990; see also Friedman and Schwartz 1963, 301).

The Destabilizing Effects of Some Stabilization Policies

Deposit insurance is typically justified on the grounds that it stabilizes the banking industry and prevents panics like those observed during the Great Depression. In the short run, this is certainly correct. With deposit insurance, depositors know that whatever happens to the bank, their money is safe. This reduces their tendency to start a run on their bank. The Great Depression provides a case in point; shortly after the creation of federal deposit insurance, the bank panics stopped. However, in the long run, deposit insurance can have the opposite effect, gradually eroding the quality of banks' asset sheets and the stability of the banking industry. This is because deposit insurance creates a moral hazard problem. Knowing that their deposits are insured, depositors (or their agents) have little incentive to monitor the prudence of their banks' investment decisions.

Richard Grossman's (1992) study of the thrift industry during the 1930s presents especially strong evidence of moral hazard. Federal deposit insurance for savings and loan companies began in 1934, with

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the establishment of the Federal Savings and Loan Insurance Corporation (FSLIC). According to Grossman, the FSLIC initially granted deposit insurance to thrift companies with the highest-quality asset sheets—those that took the least risks. Over time, though, insured thrifts that were not closely monitored and regulated engaged in increasingly risky lending activity. In a study of a state-run deposit insurance program in Kansas during the 1920s, Wheelock and Subal Kumbhakar show that banks that belonged to the state insurance program maintained lower-quality asset sheets and failed at a higher rate than banks that did not (Wheelock and Kumbhakar 1994). Other studies have uncovered similar evidence for state-run insurance programs during the late nineteenth and early twentieth centuries.[4] Moreover, while it is typically believed that federal deposit insurance was created to protect individual investors, the historical record suggests a much different story. In fact, the strongest lobbyists for the creation of deposit insurance programs, at both the state and federal levels, were small banks with low-quality asset sheets. Large, well-run banks opposed insurance because they believed (correctly) that mandatory insurance programs would effectively subsidize poorly run banks (Calomiris and White 1994; Economides, Hubbard, and Palia 1996; White 1983, 200).[5]

Other banking regulations have proven equally unsound. Selgin's point about the effects of branch-banking prohibitions during the Great Depression is made more generally by studies showing that laws limiting interstate banking and laws prohibiting banks from performing brokerage services made it difficult for banks to diversify and avoid region-specific risk (Calomiris and White 1994; White 1993). Selgin (1994) also shows that countries with free banking systems—systems that allowed banks to issue currency rather than reserving this power to the government—have been much less likely to experience bank panics than countries (like the United States) with unfree systems.[6] Even in areas far removed from finance, there is evidence that regulations ostensibly designed to promote economic efficiency have undermined stability. For example, George Bittlingmayer (1993) presents evidence that periods of unusually stringent antitrust enforcement are correlated with slumps in the stock market and reduced economic output. The mechanisms through which antitrust regulation could lower stock prices and slowed real economic activity are many. But the basic idea is this: antitrust enforcement generates un-

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certainty about the future and discourages firms from making long-term commitments .

* * *

The notion that postwar government intervention has been responsible for economic stability is, therefore, more a stylized half-truth than a stylized fact. The postwar economy is not, in truth, that much more stable than the prewar economy. The prewar Depression appears to have been caused by interventionism, not laissez-faire. And many of the policies now thought to debar future crashes may actually make them more likely. We would do well not to let pride in our enlightened interventionism lead us to exaggerate the health of the economy and our ability to regulate and maintain that health. Herbert Hoover and Irving Fisher indulged such exaggerations, and we know what history held in store for them.

NOTES

1. By contrast, FDR won the elections of 1932, 1936, 1940, and 1944 with 57, 61, 55, and 54 percent of the popular vote, respectively.
2. After the 1987 crash, the Federal Reserve promised to maintain liquidity and intervene as a lender of last resort. After the 1929 crash, the Fed did just the opposite: it reduced the money supply and refused to act as a lender of last resort. This is discussed in detail below.
3. See Romer's 1993 review essay, "The Nation in Depression."
4. See, for example, Calomiris 1990; Wheelock 1992; Wheelock and Kumbhakar 1995; and White 1983.
5. Consider what would happen if insurance companies were not allowed to charge different rates to good and bad drivers, or were forced to insure truly awful drivers. In such a case, insurance companies would have to increase the premiums for good drivers to compensate for the increased claims they would have to pay to poor drivers. In effect, good drivers would be subsidizing poor drivers.
6. Selgin (1994) argues that the positive correlation between increased government regulation and bank panics demonstrates that government restrictions undermine the health of the banking system. Critics of this view might argue that Selgin has the causality backwards. It could be that nations with frequent bank panics are more likely to pass laws regulating the banking industry. History, however, seems to suggest that Selgin has, by and large, identified the correct line of causality. See, for example, his discussion of bank panics during the Great Depression.

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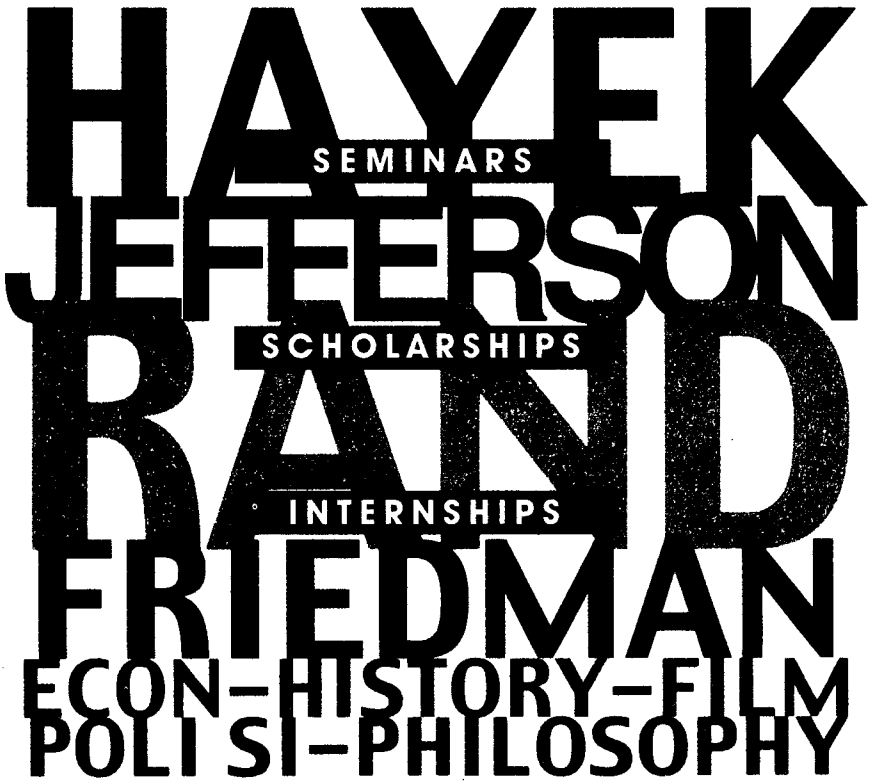

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Steven Horwitz

KEYNES AND CAPITALISM ONE MORE TIME: A FURTHER REPLY TO HILL

ABSTRACT: *Greg Hill continues to miss my distinction between what is true of free-market capitalism and what is true of the interventionist forms of capitalism that characterize Western economies. It is central banking that triggers lower aggregate demand when the public's desire to save grows. If increases in saving occur through the holding of additional private bank liabilities, rather than central-bank created money, banks could adjust their supply of liabilities so as to keep saving and investment equal and avoid reductions in aggregate demand. Hill's alternatives to market-driven financial intermediaries also fail to compare fairly the market and political processes.*

Greg Hill and I are certainly not going to settle a debate with more than 50 years of literature behind it, but I do think we can once more explore each other's positions with the hope of sharpening our understanding of the issues in question. In this response to his rejoinder ("Capitalism, Coordination, and Keynes: Rejoinder to Horwitz," *Critical Review* 10, no. 3), I will try to address the arguments I think raise the most difficulties and point toward the most fundamental issues. As was the case in his rejoinder, I will be unable to address every point I might like to, and I hope that such omissions do not suggest that I think the issues skipped are unimportant or uninteresting.

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Steven Horwitz is Associate Professor of Economics at St. Lawrence University, Canton, NY 13617, telephone (315) 229-5731, telefax (315) 229-5819, e-mail shor@music.stlawu.edu, www: http://music.stlawu.edu/shor

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Savings, Interest Rates, and Wealth

In his rejoinder, Hill (373-74) breaks down my reply into five main points. He first claims that I believe that "the banking system and the rate of interest assure that saving and investment are effectively harmonized." This way of phrasing my position suggests that I believe this result to hold at all times and in all places, or at the very least in contemporary market economies. Neither is the case. Instead, I would say that the banking system and the rate of interest *can* assure that *aggregate* saving and investment are effectively harmonized, *if the right banking institutions are in place.* But in the United States and other really existing market economies, these institutions are *not* in place. Perhaps this was not clear enough in my original reply. It is certainly a point that Keynesians have rarely addressed.[1]

Hill (374-75) further disputes my claim that an increase in savings will (with the right institutions in place) lower interest rates and signal investors to produce outputs that will come into being relatively farther in the future. Hill contends that an increase in my own savings from not purchasing, say, a dinner tonight will be offset by the savings lost from those who would have received income had I done so. Hill argues that the effect on total savings is zero, as the income and savings gained by me and my bank's borrowers and their suppliers cancel out the reduction in savings from those who don't receive additional income because I did not purchase a dinner. With no effect on total savings, there is no fall in the interest rate and no spur to investment.

Hill continues to claim, however, that savings will reduce aggregate income, presumably due to the income lost by the restaurant owner and her suppliers. But if, instead of buying dinner, I hold an additional bank liability that enables the bank to lend more, then the borrower *and its suppliers* see an increase in their income, offsetting the loss experienced by the restaurateur and her suppliers. How total income falls here is not clear. Whichever path the resources in question take, it is true that the very short-run effect will be that some people's income rises while that of others falls. However, over time, the increase in saving makes possible a shift of resources toward the capital stock, and this, in the long run, raises overall wealth.

My contention is that interest rates are not purely monetary phe-

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nomena. They are determined by time preferences and people's decisions about when to buy goods. More specifically, the interest rate is, fundamentally, a reflection of the price differentials between consumer goods (those that can be consumed in the present) and producer goods (those that must be transformed in order to become consumer goods). It is the differences between present and future prices that comprise the phenomenon of interest.[2] Saving affects the interest rate because it involves abstaining from purchases of goods now, freeing up funds to produce future goods.

In the case Hill and I have been discussing, the decision not to purchase dinner tonight puts downward pressure on the price of a consumer good, a dinner, while the loan that my bank makes with my saving puts upward pressure on the prices of the producer goods that the borrower purchases with it. It may well be true that in the short run, the total amount of income does not change, but the increased desire to save on the part of some market actors will cause changes in the relative prices of consumer and producer goods, lowering interest rates. Today's movement away from consumer goods toward producer goods will tomorrow cause an increase in income due to the increased investment in production processes with a larger number of intermediate stages. As Hayek (1941) and many before him argued, a lengthier structure of production generally produces more value. No one believes that increases in saving *instantaneously* increase wealth. In the short run the effects may be a wash, but over time, increases in saving lengthen the structure of production and thereby increase wealth.

Hill's (375) response to this argument is to claim that that a generalized movement to more saving will produce not a "piling up of bank deposits" but a general fall in income. Everyone's attempts to reduce spending will mean a fall in everyone's income, as income and spending are two sides of the same coin. The problem with this argument is that it continues to ignore the question of where the savings "goes." If one reduces one's spending, the resources must go somewhere other than toward spending. If they remain in one's bank account, then bank deposits are greater than they otherwise would have been. As a result, banks have more to lend and that lending and the spending by the borrowers replaces the income "lost" through the saving. However, if the saving takes the form of people holding more currency and coin, then Hill is correct: bank balance sheets will contract, and spending and income will fall. However, as I will argue

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below, this is not a flaw in capitalism in general, but the result of poor banking institutions, resulting from various government interventions in the market rather than from intrinsic market forces. These interventions make the withdrawal of base money more likely than it needs to be, by creating conditions where hand-to-hand currency doubles as bank reserves.

Price Flexibility and Unemployment

The second point I raise, according to Hill, is that "prolonged periods of unemployment cannot occur if wages and prices are flexible." Once again, the matter is more complex than that statement of my position might lead one to believe. If the statement were to read "if wages and prices are *perfectly* flexible," where "perfectly flexible" includes perfectly rational agents, then yes, I would agree. But such a hypothetical is irrelevant to the real world. Although making prices and wages more flexible is a good thing, there is no way to make them flexible enough to be able, *by themselves*, to avoid prolonged periods of unemployment. Prices in neither real-world market economies nor any market economy inhabited by human beings as we know them will be completely and perfectly flexible. For one thing, human beings are not as rational as a perfectly flexible price model would require them to be; and for another, there are the sorts of collective action problems that Hill identifies (377). Real-world prices will always be somewhat sticky, both upward and downward.

For just this reason, it is important to avoid monetary disequilibria that require economy-wide adjustments in prices. Excesses or deficiencies in the money supply, in the presence of imperfectly flexible prices, can certainly cause prolonged periods of unemployment, as the Great Depression and the stagflation of the 1970s demonstrate. Hill's points (377–78) about fixed debt-service contracts and the resulting cumulative rot are all possibilities if the money supply is deficient (or, put differently, if planned saving exceeds planned investment). I do not dispute any of that. My concern is over what causes the mismatch between saving and investment. That is the core of the debate, and it has everything to do with the economy's banking institutions and their ability to respond to changes in time preferences and the demand for money.

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Money's Uniqueness and the Importance of Banking Institutions

This issue nicely segues into the third point on Hill's list, my claim that "money is not different from other goods." Once again, this statement dramatically oversimplifies my argument. I said: "Cash is not *fundamentally* different than other goods and services; if the public wishes to hold more of it, there is no reason that the laws of supply and demand *should* take a holiday" (Horwitz 1996b, 364, emphases added). Hill says of this that I do "not trouble to tell us why people wish to hold more cash when they can hold interest-bearing securities instead. If he did, his time-preference theory of the interest rate would fall to pieces" (Hill 1996, 378). But in fact, I did: "Cash is no different [from other goods that provide a particular service], in that it provides the service of being available to buy things" (Horwitz 1996b, 364). CD players provide music-playing services; cash provides availability services. Cash, like other goods, provides services to those who hold stocks of it. Although I didn't say the magic word "liquidity," I take the "service of being available" to be the same thing. In order to be "available" to buy things on the whim of the holder, a good must be perfectly liquid; and money's perfect liquidity is the one important difference between money and other goods.

I also didn't mention the tradeoff of money against interest-bearing securities, although I have addressed that issue thoroughly elsewhere.[3] Indeed, money does allow, in Hill's words, "holders to postpone decisions"—this is what it means to provide the service of being available to buy things. The holding of money for its availability services, and the fact that interest-bearing securities are inferior along these lines (we cannot spend Treasury Bills at the grocery store), are perfectly consistent with a time-preference theory of interest.

Hill is implying that once one admits that cash trades off against such securities, one has to admit that the demand for money is caused by interest rates and that, therefore, the interest rate is purely or overwhelmingly a function of the supply and demand for money. However, the fact that I prefer the present to the future is intimately linked with the fact that the future is uncertain. Contrary to Hill's claim (379-80), I never denied that people hold money due to uncertainty. In fact, that is precisely why they hold it, as I have argued elsewhere (1992, ch. 1). The desire for liquidity or availability services is a manifestation of the more fundamental concept of time-preference. If

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I choose to sell bonds and acquire cash, this suggests that I am worried about the future and want cash so as to leave my options open. My time preferences have shifted toward the present. When I sell my bonds, bond prices fall and interest rates rise, as they should to reflect my concerns about the future. There is no contradiction between recognizing money's liquidity or availability services and a time preference theory of the interest rate.

However, there is a more fundamental issue that Hill misses. After his claim that money is in fact different from other goods due to its liquidity properties, he writes:

> When the demand for an ordinary good increases and its price rises, labor can be employed to produce more of it. But when the demand for money increases, private enterprise cannot (legally) employ labor to increase its supply. In this respect, the "laws of supply and demand" do, indeed, "take a holiday." (379)

But this is precisely what is at issue. Under the current set of monetary institutions, Hill is correct; but my point in saying that there's no reason that supply and demand *should* take a holiday was to suggest that there are alternative banking institutions that *do* enable private enterprise to produce more currency and deposits when the public wants them. If so, then the downward effects on aggregate demand of an excess of planned saving over planned investment will be avoided. (Of course, if a central bank is wise enough to respond appropriately, it can avoid those effects as well.)

Hill (377) claims that I "do not think that when planned saving exceeds planned investment . . . unemployment will result." That is false as written. With a central banking system and less than perfectly flexible prices, such imbalances between saving and investment can indeed cause unemployment. My argument is that when such imbalances occur due to changes in either saving or investment, *the right set of banking institutions* can adjust the interest rate in order to bring the two quantities closer and avoid anything more than very minimal, very transient macroeconomic consequences.[4]

He then argues (377) that, for example, a fall in the demand for cars due to an increase in saving will not lead car producers to plan for an increase in future demand (and thus increase their demand for producer goods), as they are currently facing falling demand, falling prices, and excess capacity. What Hill misses here is that, under the

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right set of banking institutions, the increase in saving will lower interest rates and reduce the cost of investment for car makers. Even though prices and demand are lower, so will be costs. Under such circumstances, it is not clear that more investment would be unprofitable. In addition, car makers need not make the crude extrapolations Hill attributes to them. Just because prices are falling today need not lead them to believe that future prices will be low. Entrepreneurs who (correctly) interpret the lower interest rate as signaling a relative shift in demand from the present to the future will take lower prices today to mean relatively *higher* prices tomorrow.

The issue of alternative banking institutions also comes into play when Hill notes that "to hold cash is to take refuge from uncertainty." He then quotes G. L. S. Shackle, who argues that taking refuge from uncertainty (presumably by holding cash) means that we also take refuge "from enterprise, from the giving of employment." There are two questions here. First, does Hill mean "cash" or "money"? If Hill is referring to cash in the strict sense (that is, base money), then he is correct.[5] Withdrawals of base money from any banking system will generate contractionary effects by reducing the ability of the banking system to lend. However, if he means money more generally, this is not the case. In the short run, my decision to hold more money in the form of bank deposits redistributes employment among different people, but does not reduce "the giving of employment." And in the long run, my uncertainty-induced decision to hold more bank deposits will generate increased employment.

One problem with central banking is that the paper money that people use for hand-to-hand transactions (currency) is also part of bank reserves and the monetary base. Thus, when the demand to hold currency rises relative to deposits, there is a withdrawal of base money (bank reserves) and a resulting contraction in bank lending and in the economy, unless the central bank offsets the change with an injection of reserves. In really existing capitalism with central banks, an increased relative demand for currency due to uncertainty can indeed touch off a decline in aggregate demand (assuming anything less than perfectly flexible prices) if the central bank does not react properly. However, in a banking system where individual banks were allowed to create their own currencies in the same manner as their deposits, this would not be the case.[6] Where currency is a liability of the banking system, taking refuge from uncertainty in currency is no different from taking refuge in bank deposits and the effects are

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the same: an increase in wealth over time, due to the increase in saving. Of course, in such a system, an increased demand for base money (which might or might not be a precious metal) will have contractionary effects, but that is true of *any* banking system. The question is whether fallible central bankers with poor feedback mechanisms, as opposed to fallible individual bankers with market signals to rely on, will do a better job in adjusting the total size and composition of the money supply to changes in demand. By making currency a bank liability rather than a reserve medium, a "free banking" system would allow one less way for the public to withdraw bank reserves and cause a major contraction in credit and aggregate demand. The problem with contemporary central-bank-controlled capitalism is the very fact that when the public wants more of certain kinds of money, private enterprise cannot legally "produce more of it." Such a prohibition is not an irremediable fact of the world, but a particular institutional condition of modern capitalism that could be changed by ending government control over money.

The Investment-Saving Circularity

The fourth point Hill attributes to me is the view that "income is the source of expenditure." He got this one right. However, his supposed counterexample does not do the work he thinks it does. He argues that some expenditures are financed by lines of credit extended by a bank and not by anyone's savings, such that "contemporary banks create credit in excess of savings, which means that 'savings proper' need not precede investment" (381). This is simply untrue. To the extent that competitive banks *themselves* create credit, they can do so only if they have excess reserves to back it up, or if the public is willing to hold balances of the bank's liabilities. Both of these are forms of saving.

Suppose a bank decides to extend a $1 million line of credit to a firm. When the loan is spent, the recipients (the sellers of the plant and equipment purchased by the borrower) deposit the checks written against the loan by the borrower, either at their own banks or at the bank that lent the firm the money in the first place. If all of the spending winds up with customers of other banks, the lending bank must have $1 million in excess reserves on hand to meet its clearing needs at the central bank. In that case, it cannot lend without already

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having sufficient reserves on hand to withstand the demands made on it during the clearing process. That is, the reserves (which constitute saving to the extent that they arrive at the bank via deposits made by their customers) must be on hand before the line of credit (investment) is spent. And if some portion of the spending comes back to the lending bank in the form of deposits made by its own customers, these deposits reflect the willingness of the public to hold the bank's deposit liabilities. This, too, is a form of saving, in that depositors are forgoing expenditure so as to provide banks with funds to loan out to their borrowers. In this case as well, investment must be financed by savings.

Hill rightly notes that there are cases in which banks can lend more than the public is willing to save. This is called inflation. But only a central bank, whether directly run by the state or in possession of state-granted monopoly privileges, has the power to create bank reserves *ex nihilo*; in doing so it gives banks the ability to lend without any prior voluntary saving by the public. Of course, the spending power that comes with such loans has to come from somewhere. This magic power comes from diluting the value of the real cash balances of other money holders when inflation leads to higher prices (a process also known as "forced saving"). In such a case, banks can indeed lend without the public wanting to save, the interest rate will indeed be sending an erroneous signal about the time preferences of the public, and unemployment will eventually result.[7] But this is not the fault of capitalism. It is a product of profoundly nonmarket institutions—central banks. Central banks can and do generate the kinds of system-wide discoordination that Keynes and Keynesians wrongly believe are inherent in capitalism.

Hill also claims that despite my argument to the contrary, entrepreneurs "in the aggregate sometimes overestimate demand" (381). I admit to not being clear on this point, so let me try to correct that. I do *not* deny that entrepreneurs might make errors in the aggregate. In fact, they *will* do so when market signals, in particular the interest rate, are distorted by well-intentioned but counterproductive government intervention, such as the case described in the previous paragraph. Such system-wide error is what we mean by a recession. My original claim was that "idleness caused by entrepreneurial error [as opposed to distorted prices] would show no systemic patterns, being an irremovable feature of a world of uncertainty" (Horwitz 1996b, 367). The burden on Hill is to offer an example where entrepreneurs

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make aggregate errors that are sustained for a long enough period to cause a recession, but *where the errors are not the result of distorted prices or incentives created by government intervention.* Otherwise he has not shown that free-market capitalism (as opposed to the really-existing interventionist forms of capitalism of the Western world) shows any tendency to system-wide error.

Hill's Alternatives to the Market

The fifth point to which Hill responds is my claim that "real-world markets may be imperfect, but they are superior to every alternative" (Hill 1996, 374). Hill's response is to offer two alternatives that he believes would be superior to the market. The first is "a computer network linking the economy's thousand largest firms with a Walrasian auctioneer at the center of the network" (383). The auctioneer would coordinate each firm's investment demands with the various levels of investment desired by the others. There are numerous problems with this proposal. It appears that, for Hill, the problem with the Walrasian model is not that it isn't a good description of what an economy *should* look like, but that it just doesn't describe existing economies very well. Therefore, we should create institutions that make economies look more like the model of Walrasian general equilibrium. However, the conditions (perfect knowledge, stable preferences, etc.) necessary for anything close to general equilibrium to exist in the real world are so far removed from the reality of human existence that using it as any kind of goal or end-state is sure to lead to erroneous public policy.

Second, this proposal involves no mention of the interest rate whatsoever. Why wouldn't firms just pick any old dollar amount for their desired investment? Hill suggests that firms that do not meet their investment commitments would suffer tax penalties. But how are firms to begin to know how much to invest without a market interest rate (driven by meaningful supply and demand decisions) to guide them? To first blind firms and then penalize them for not seeing correctly hardly seems feasible. How will our auctioneer know how much investment is possible without some knowledge of the public's time preferences, in the form of its willingness to save? During the cycles of negotiation needed to converge on the Walrasian solution, how will our computerized auctioneer assure that the data in-

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forming those investment preferences do not change?[8] Where will the resources come from to finance the level of investment that the auctioneer determines is appropriate? Hill claims that households will "remain free to save as they wish" (384), but what if they choose to save less than the auctioneer determines is the "socially" appropriate level of investment? Will the central bank make up the difference? If so, what about inflation and its deleterious consequences?

Third, this proposal assumes that the criteria for the investments that firms wish to make can be quantified and articulated in a form that can be fed into a computer. But much economic information is not known in forms that can be articulated or quantified. A significant component of our knowledge is "tacit" and can be revealed only in real social actions, not in words or numbers. Much of what goes on in the market is action driven by knowledge that the actors themselves cannot articulate. The experience acquired from operating in a particular market context often leads producers to make decisions that they themselves might describe as educated guesses or even hunches. They may not be able to specify their exact reason for thinking a certain new product or new line of business is the right thing to do, but they have that "gut feeling." The ability to buy and sell in a market enables them to make use of this knowledge without recourse to a linguistic or statistical rationale. As a result, any proposal to coordinate economic activity centrally, whether by means of language or computers, is doomed to rely on even less complete knowledge than is available in imperfect markets.[9]

Finally, Hill once again overlooks any public-choice considerations. Computers are programmed and operated by real human beings with their own ideas and interests. With billions of dollars at stake, the programming of such computers would be fertile ground for rent-seeking by private firms and vote- and power-seeking by political actors. Hill's response to an imperfect market is to assume a perfect political process.

His second proposal is to create a market in which "workers could insure the value of their human capital by purchasing an insurance policy that paid a benefit in the event the average wage of their occupational group fell below a certain threshold. Similarly, imagine new insurance markets in which people could hedge against changes in their income that were due to changes in national income over which individuals have no control" (384). My reaction to this is that if

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such policies are to be created by private insurance providers, more power to them. This is not to suggest that since such policies haven't been offered, they must be a bad idea. It's possible that no one has thought of them before. But there are reasons for skepticism.

First, what happened to the uncertain future? Hill's proposal depends crucially on the ability of insurance sellers to quantify the unknowable. Life, health, and auto insurance premiums can be computed on the basis of past performance and the statistical likelihood of similar events in the future. Those are largely quantifiable risks because most of the causal factors are well known, as is their relationship to the insured-against outcome.[10] But Hill proposes insurance against the possible effects of future changes that we cannot now even imagine. As Austrian economists are fond of saying, the market is a discovery procedure. Israel Kirzner (1989, ch. 2) in particular has emphasized that the market discovers knowledge of which we were previously ignorant. We do not know what it is we do not know. (The value of human freedom rests on the fact of our ignorance of the future.)[11]

In the face of this structural ignorance, how could sellers of insurance calculate the risk of future changes in wages? After all, such changes are almost totally dependent on human choices and the link between those choices and eventual outcomes is beyond our predictive powers. What goods will people be buying in the future? What new inventions will arrive that will overturn the existing set of wages? Could an insurance company in 1965 have computed the risk of changes in the wages of U.S. auto workers? Could it have possibly foreseen the rise of Japanese competitors? Of computer and robotic technology? How will insurers be able to assess the possibility of alternative effects on the multitude of wages that comprise the market process? I find it incredible that Hill, whose Keynesianism is built on the uncertainty of the future and the inability of market actors to penetrate it successfully, would now believe that insurance companies could make a profit providing such policies.

Hill also overlooks the fact that a decline in wages for a particular group might be good for society as a whole. It was not a bad thing that the wages of candlemakers fell with the invention of the light bulb. If resources are used to compensate those whose wages fall every time there is a socially beneficial change in technology or the organization of production, are we not simply cancelling out the benefits of such changes and perhaps reducing the number of them

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forthcoming? Although it is often true that such innovations reduce the wealth of particular producers and workers in the short run, the long-run increases in the standard of living that result are to the benefit of all. A policy that would deter techonological innovation and productive efficiency will, in the long run, harm everyone, including those it is explicitly trying to help.

Hill may have government provision of insurance in mind here. Such a program would still face the technical problem of computing risk, and the social problem of justifying the resulting redistribution. But government provision would get around the barrier to implementing such a plan posed by the difficulty of making accurate predictions. One wonders, however, whether the waste generated by mispricing such insurance by the government wouldn't more than offset the potential gains. Once again, the public-choice issues surrounding government provision are nowhere to be found, nor are any mention of the undesirable unintended consequences of governmental support of inefficient institutions.

* * *

Hill and I agree, I think, that if planned savings and planned investment remain out of balance for an extended period of time, a fall in total income and employment is likely to follow. This agreement points to the influence of turn-of-the-century macroeconomic thought on both Keynesian and Austrian economists. We disagree about the *source* of these imbalances. Hill, and most Keynesians, believe that these imbalances are inherent in free markets because of their decentralized decision making. I argue, by contrast, that such mismatches either stem from government interventions in the market (especially in the banking sector) or are prevented from correcting themselves with a minimum of disruption as a result of such interventions. Banking institutions free of the practices and policies imposed by government control of money would be able to translate changes in the public's desire to save (in the form of changes in their willingness to hold bank liabilities instead of purchasing goods and services) into investment lending, with no greater macroeconomic consequences than markets experience from a change in the demand for any other good or service. Keynes's "general" theory of employment, interest, and money is instead a rather specific theory, useful, at best, in explaining one way in which recessions might occur not under capitalism per se, but under interventionist forms of capitalism.

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The recessions and depressions experienced by the United States all took place under this special set of circumstances. Before claiming such examples as evidence for the failures of capitalism, one must consider whether it was government control of the banking system (whether at the federal or state level) that might have been the source of the problem.

Notably, Hill points to no empirical examples to back up his theoretical argument. I would argue that extensive recent research in American financial history offers strong evidence that government intervention was the culprit in all such instances.[12] Thus, there is neither theoretical nor empirical reason to believe that unregulated market economies would lack self-regulatory properties sufficient to avoid recessions brought on by changes in the public's desire to save or invest.

NOTES

1. See my discussion of Keynes and free banking in Horwitz 1989.
2. For example, see Mises 1966, 524: "As the consumer goods are present goods, while the factors of production are means for the production of future goods, and as present goods are valued higher than future goods of the same kind and quantity, the sum thus apportioned . . . falls behind the present price of the consumers' goods concerned. This difference is the originary interest." Of course, it should be noted that actual market rates of interest include factors other than what Mises is terming "originary interest." Rates of inflation, uncertainty about the future, borrower-specific risk and many other factors can cause market rates of interest to differ from these intertemporal prices. What is important is that time preference is necessary and sufficient to generate the phenomenon of interest.
3. See Horwitz 1990. Maclachlan 1993 discusses the similarities between Austrians and Keynesians on these issues.
4. As my work in macroeconomics is squarely in the Wicksellian tradition, it would be an odd thing indeed for me to claim that a disequilibrium between ex ante savings and investment would not cause unemployment. See Horwitz 1996c for an elucidation of this theoretical perspective.
5. Hill frequently uses "cash" and "money" interchangeably and appears to overlook the important differences between the two. Normally, "cash" refers to base money such as currency and coin, while "money" is a broader concept that includes cash plus the variety of cash substitutes banks produce, such as savings and demand deposit accounts.
6. Such a system has been explored in the recent literature on free banking. See, for example, Selgin 1988 and White 1996. It is also worth noting that

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the possibility of bank-created "currency" may arrive in the form of so-called "smart cards" and "e-cash" through computer technology and the Internet. See Dorn 1997 for a collection of papers addressing these issues.

7. This is the fundamental idea behind the so-called Austrian theory of the business cycle, which is most clearly presented in Rothbard 1963, ch. 1.
8. For these reasons and others, this scheme falls victim to the same sorts of objections that F. A. Hayek (1940) raised against Oskar Lange's market socialist proposals to determine the equilibrium prices of capital goods by means of central planning. See also Lavoie 1985.
9. For an application of the tacit knowledge argument to the claim that computers can coordinate economic decisions better than the market, see Horwitz 1996a.
10. In addition, there may be some very difficult moral hazard problems here. If I have insurance against a fall in the average wage of my occupational group, what is to prevent me from slacking off or unionizing against technological changes that might affect my group's wages? A group's average wage can be affected by the actions of the insured. How would the providers of such policies monitor the behavior of those insured, so as to avoid them bringing on the insured-against outcome?
11. The analogy to intellectual freedom in the academic disciplines is a very strong one. The value of academic freedom derives from the fact that we do not know what will be discovered, so we need to keep all possible avenues of discovery open.
12. There has been a great deal of recent scholarship exploring the relationship between state intervention and the numerous problems that litter the history of American banking. For a sampling of this work, which covers everything from the early nineteenth century to the bank and savings and loan failures of the 1980s, see the following: Dowd 1992, Ely 1988, Horwitz 1990, 1992, Mullineaux 1987, 1988, Rockoff 1974, Rolnick and Weber 1983, Selgin 1992 and 1994, Selgin and L. White 1988, Timberlake 1993, E. White 1983, and L. White 1993.

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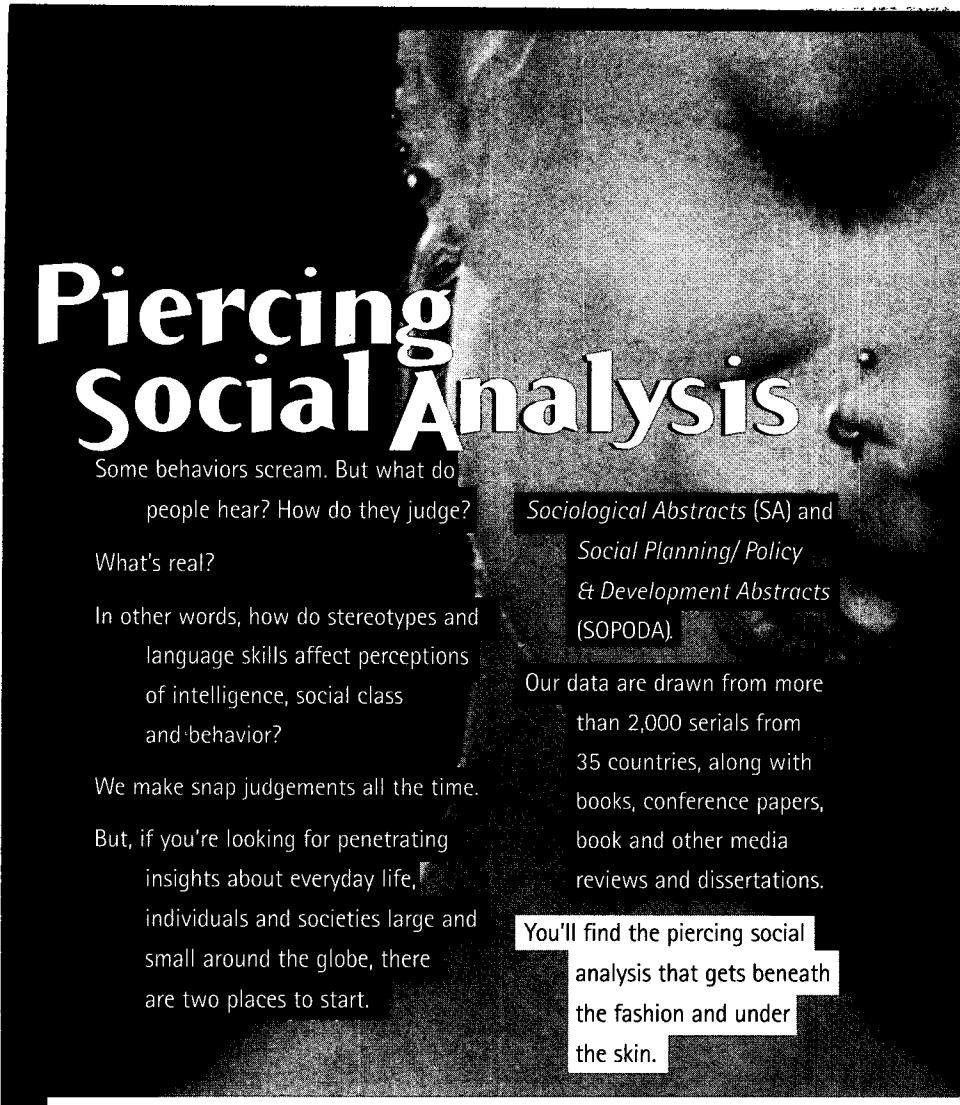

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Greg Hill

AN ULTRA-KEYNESIAN STRIKES BACK: REJOINDER TO HORWITZ

ABSTRACT: *In real-world markets, individual intentions cannot be brought into perfect harmony before decisions are taken. Choices made without this pre-ordering—choices made in ignorance of one another—produce unwanted outcomes. It is this absence of coordination among plans, not centralized banking, that is the primary cause of macroeconomic market failure. Steven Horwitz's free-banking alternative would aggravate the collective-action problems inherent in economies without complete markets.*

Professor Horwitz describes "the core of the debate" between us as the issue of "what causes the mismatch between saving and investment" (5). I would characterize our disagreement more broadly as a late round in the long-running debate about the effectiveness of real-world market economies in coordinating decentralized decisions. Horwitz is convinced that centralized banking is the chief obstacle to a rational order of individual choices, and that this ideal can be brought within reach through the institution of "free banking." I shall argue that coordination failures originate mainly in the void created by the absence of complete markets. When decisions must be taken in ignorance of one another, they often give rise to collective action problems that are beyond the power of markets to solve. Free banking would not ameliorate these problems, but exacerbate them.

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Greg Hill, Seattle Office of Management and Planning, 300 Municipal Bldg., Seattle, WA 98104, telephone (206) 684-8049, telefax (206) 233-0085, e-mail greg.hill@ci.seattle.wa.us.

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I. THE ACT OF SAVING

In *The Moral Economy: Keynes's Critique of Capitalist Justice* (*Critical Review* 10, no. 1), I developed some of the ramifications of Keynes's argument that saving and investment are not effectively harmonized in real-world market economies, and I tried to show why this discordance casts doubt upon justifications for unfettered capitalism. In his initial reply to my paper, Horwitz claimed that while "real-world banks and interest rates do not operate perfectly . . . they do effectively facilitate intertemporal coordination day in and day out" (1996, 359). In his present response, Horwitz adds the stunning proviso that "the rate of interest *can* assure that *aggregate* saving and investment are effectively harmonized, *if the right banking institutions are in place*," viz. free banks that are permitted to create their own currency and deposits (1, Horwitz's emphasis). The economic arrangement Horwitz wishes to defend—free markets with free banking—comprises a different economic system than the actual capitalist economies Keynes analyzed. In outlining some of the causes of macroeconomic market failure, I shall try to keep before us Horwitz's claim that these coordination problems would not arise, or would at least be less severe, under laissez-faire banking than under centralized banking.

To begin, let us consider Horwitz's claim that, with free-banking institutions in place, saving and investment decisions would be effectively coordinated, a contention Horwitz says "Keynesians have rarely addressed" (ibid.). The problem of coordination arises because an individual act of saving—a decision not to buy dinner today—does not necessitate a decision to purchase any particular good at any particular time in the future. Thus, the act of saving—literally, not spending—depresses current demand without stimulating the production of goods that may be consumed in the future (Keynes 1936, 210).

In response to this argument, Horwitz claims "that an increase in savings will (with the right institutions in place) lower interest rates and signal investors to produce outputs that will come into being relatively farther in the future" (1). This reduction in interest rates requires, on Horwitz's own theory, that the "increase in savings" be an increase in *total* savings. The question is not whether an individual can increase her own saving by reducing her spending, but whether this act of thrift will increase total saving. The rub, of

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course, is that when one person reduces her spending, someone else's income must fall, and there is no guarantee that the increased saving of the former will not be offset by the decreased saving of the latter.

Horwitz no longer denies this, but nevertheless insists that "the increased desire to save on the part of some market actors will cause changes in the relative prices of consumer and producer goods, lowering interest rates" (3). Now, we may grant that when the public reduces its consumption spending, the price of consumer goods may fall. But Horwitz still owes us an explanation of why the relative price of producer goods will rise. To claim that an increase in the flow of saving signals a rising demand for goods in the future begs the question, for it is the assumed increase in total saving that is in doubt. To claim, on the other hand, that the price of producer goods rises because the interest rate has fallen is to reason in a circle, for Horwitz has just argued that lower interest rates are due to the relative rise in producer-goods prices, not vice versa.

Horwitz speaks of "the increased *desire to save* on the part of some market actors" (ibid., emphasis added), but fails to grasp the difficulty involved in communicating this desire to producers. For an individual, the decision to save involves a reduction in present consumption for the sake of consumption later on. And if, at the time this person reduces her current consumption spending, she were simultaneously to place an order for goods to be delivered in the future, there would be no coordination problem; firms could reduce their output of consumer goods and increase their output of producer goods knowing there would be greater demand in the future. But while a reduction in consumer spending *makes possible* a shift of resources from consumption to investment, this act of saving provides firms *no inducement* to expand future output in the absence of an order for future delivery.

Free banking would do nothing to mitigate this difficulty. The coordination problem Horwitz and I have been debating does not arise because real-world banks are constrained by the reserve requirements mandated by a central bank; it arises because the individual act of saving conveys no information to producers about *what* will be desired in the future or *when* it will be desired. Allowing banks to create their own currency and deposits would not eliminate this problem because it is the *demand* for investment loans that is lacking, not the *supply*!

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Horwitz's explanation of the supply side of the credit market is not quite right either. Although Horwitz has corrected his original oversight, wherein he counted the increased saving of Keynes's abstinent diner without counting the decreased income and saving of the restaurant owner, he now makes the same mistake with respect to the bank deposits of the two parties. Thus, Horwitz asserts that when the diner foregoes his regular meal at the neighborhood restaurant, "bank deposits are greater than they otherwise would have been" (ibid.). This conclusion, however, is based on an incomplete accounting of the *net* change in *total* bank deposits. For while Horwitz recognizes the increase in deposits at the diner's bank, he fails to take account of the decrease in deposits at the bank of the restaurateur, whose income and savings are lower because the former restaurant patron is no longer buying dinner. Thus the new loans made by the diner's bank, and the spending and income they generate, which Horwitz notices, must be offset by the decrease in loans made by the restaurateur's bank and the consequent reduction in spending and income, which Horwitz overlooks.

Finally, let us consider the question Horwitz believes Keynesians have dodged for 50 years, a question that if squarely faced, Horwitz insists, must restore our confidence in the market's ability to coordinate saving and investment decisions, i.e., "the question of where the savings 'goes'" (3). As a prelude, it may be noted that this question loses much of its critical force once it has been shown that the individual act of saving ensures neither an increase in total saving, nor an increase in total bank deposits. Bearing these points in mind, let us by all means give Professor Horwitz his answer. When planned saving exceeds planned investment, firms will have produced more goods than households wish to buy at prevailing prices. Consequently, prices will fall, and firms will suffer unexpected losses. Those households that have reduced their spending will have increased savings, and these savings will "go" to firms that must cover their losses.[1] Businesses incurring losses can only meet expenses by drawing down their cash reserves, selling assets, or borrowing. Households with increased savings may acquire these assets or make deposits that support commercial lines of credit. In microcosm, the extra savings of Keynes's abstinent diner might well be borrowed by the restaurant itself—not for the purpose of investing in a new oven, but to meet expenses that cannot be met following the unforeseen reduction in the restaurant's sales revenue.

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II. DEFLATION AND THE LIMITS OF FREE BANKING

Although I have argued that an individual act of saving will not necessarily increase either aggregate saving or the total stock of bank deposits, this does not mean that the rate of interest will be unaffected by a general reduction in spending. On the contrary, when incomes and prices fall, households and firms require smaller cash balances for transaction purposes. The rate of interest will then decline, not because the price of producer goods has risen in relation to the price of consumer goods, but for the more mundane reason that banks can acquire what is now excess cash by promising lower interest payments than would have been required before the decline in aggregate income.

That said, it must be added that falling interest rates may not be sufficient to revive investment in a deflationary environment. Falling prices reduce profit margins, and it is gross profits, not bank loans, that finance the lion's share of capital spending. Moreover, deflation causes bankruptcies, which increase the risks of *both* borrowing and lending. And, finally, there is a lower limit beneath which the rate of interest cannot fall, both because of the large risk involved in holding very high-priced bonds and because investment project risks are "double-counted," once for the borrower whose project may fail and once for the lender whose loan may not be repaid (Meltzer 1988). Horwitz believes free banking would successfully counterbalance these deflationary forces. If banks were allowed to "produce more currency and deposits when the public wants them," then, Horwitz claims, "the downward effects on aggregate demand of an excess of planned saving over planned investment will be avoided" (7).

Horwitz's case for "free banking" does not come to grips with the fact that an effective banking system requires the management of collective action problems that, by their nature, cannot be solved by the independent action of many unfettered banks. When an individual bank makes a loan, the borrower will often pay away his deposit to the customers of other banks, which, in turn, will find their position strengthened to the same degree the lending bank is weakened. A bank's decision as to whether a loan will be approved or disapproved is not, therefore, a strictly private matter between the bank and the loan applicant. Rather, one bank's lending creates an external benefit for other banks. In Keynes's words, "every movement forward by an individual bank weakens it, but every such movement by one of its

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neighbor banks strengthens it; so that if all move forward together, no one is weakened on balance" (1930, 26).

If there is no central bank to manage these externalities, the banking system will be prone to instability because any change in the volume of lending is likely to be self-reinforcing. On the one hand, the increased lending by one bank will increase deposits at other banks, which are then likely to increase their lending, which further increases deposits, and so forth. On the other hand, a reduction in the lending of one bank will reduce the deposits and, therefore, the lending of other banks, which will further reduce total deposits within the system. In the latter case, each bank's contraction of credit will not only reduce the deposits of other banks, it will also increase the risk of default throughout the economy. Depending on the speed with which this contraction unfolds, a chain reaction of bankruptcies can become a "run" on the banks. Without a central bank standing ready as "the lender of last resort," the outcome of this cumulative process is unlikely to coincide with Horwitz's cheerful scenario, which contemplates nothing more than "very minimal, very transient macroeconomic consequences" in response to reductions in aggregate demand (7).

The collectively self-defeating actions of Horwitz's free banks during a credit crisis exacerbate a parallel collective action problem in the non-financial sector of the economy. Just as one bank's reduced lending weakens other banks by diminishing their deposits, so one firm's reduction in investment spending diminishes the sales and income of other firms. If, by contrast, a large number of firms were to increase their level of investment simultaneously during a slump—that is, if their investment decisions were coordinated—all would then benefit from the income and multiplier externalities generated by their cooperative investment expenditure (see Hill 1998). But when each firm, aiming to minimize its risk in a troubled economy, waits for the upswing before investing, the result is the same as when each bank passively waits for an increase in its deposits before increasing its lending: an economy stuck in an underemployment equilibrium.

III. RATIONALITY, DECENTRALIZATION, AND REAL-WORLD PRICE SYSTEMS

In his initial reply, Horwitz claimed that "with wage and price flexibility," real income and output "will not be affected" by an increase in

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the demand for money (1996, 364). Here, too, Horwitz believes a significant qualification is in order. Thus, he now insists that if "prolonged periods of unemployment" are to be avoided, prices must be "*perfectly* flexible, where 'perfectly flexible' includes perfectly rational agents" (4, Horwitz's emphasis). A certain sort of Keynesian might be tempted to respond to this astonishing concession by simply saying, "I rest my case." But I am going to resist the temptation because the concession, itself, is based on a misunderstanding.

The argument of Keynes's *General Theory* is not that prolonged unemployment is possible because prices and wages are rigid. Rather, a market economy can get stuck below the full-employment level of output because, among other reasons, unemployed workers have no means of communicating to producers as a whole their willingness to purchase additional goods *if they were again employed*.[2] The trouble in this instance is not the slow speed of price adjustments, but the absence of a market in which conditional intentions can be pooled, and all mutually advantageous trades revealed and consummated.

In the Austrian theory Horwitz affirms, prices provide the information necessary for rational decision making as well as for the coordination of individual decisions within the economy as a whole. This was, of course, the principal argument of the Austrian school in the great debate over the possibility of rational calculation under socialist planning. The problem illustrated above—that is, the incompleteness of markets in real-world economies and, hence, the absence of prices for many important kinds of goods, including goods to be delivered at future dates under contingent circumstances—dims the action-guiding lights that emanate from the price system. Kenneth Arrow, who described in exquisite mathematical detail the necessary conditions for efficient markets, concludes that "the very concept of rationality is threatened" when markets are incomplete and individuals must predict what a great many other decision makers will do (1986, 203).

The problem of imperfect information in real-world markets is often aggravated by its uneven distribution among market participants.[3] In the credit market, for example, potential borrowers often have better information about their credit-worthiness than do lenders. In this case, two of Horwitz's main themes collide with ironic force. On the one hand, the development of human capital takes time, which requires the sacrifice of current, for future, income. On the other hand, the credit necessary to meet current expenses

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may be available only to families that have already accumulated considerable wealth because, in the absence of other reliable information about a borrower's credit-worthiness, collateral is usually required in order to get a loan. The result is an inefficient pattern of investment in human capital, which also reinforces existing inequalities of wealth and income (see Shah 1992). Government-insured loans can improve upon this outcome by pooling risks, but free banks, operating in markets with information asymmetries, have neither the ability nor the incentive to do so.

IV. LIQUIDITY PREFERENCE, TIME PREFERENCE, AND THE RATE OF INTEREST

Horwitz's case for free banking rests on an exceedingly narrow conception of liquidity, which captures almost nothing of what Keynes had in mind by the term. Consider Horwitz's claim that "the desire for liquidity or availability services is a manifestation of the more fundamental concept of time-preference" (6). "If I choose to sell bonds and acquire cash," Horwitz says, "this suggests that I am worried about the future and want cash so as to leave my options open. My time preferences have shifted toward the present" (ibid.). This is a muddle. If I suddenly sell bonds to acquire cash for a vacation, then one might plausibly infer that my time preferences "have shifted toward the present." If, however, I sell bonds because I think interest rates will rise and bond prices will fall, this increase in my liquid holdings does not reflect a change in my time preferences, but a change in my forecast of future bond prices! Even if I were to sell bonds because I have become less confident about the direction of bond prices, this has nothing to do with an increased preference for current versus future consumption. Rather, it reflects my disquietude about the future course of interest rates.

In Keynes's theory, liquid assets are those that can be used as a means of payment, or can be easily converted into a means of payment, *without capital loss*. Cash and checks satisfy this requirement, and short-term Treasury Bills come very close. Long-term bonds, stocks, and specialized capital equipment are less liquid by turns. Keynes's theory of liquidity preference gives an account of the motives that influence decision makers in allocating their wealth among these different kinds of assets. For example, bondholders who become anxious about interest rates will unload their bonds in favor of more liquid as-

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sets, provided they can find willing buyers, e.g., other wealth holders who are more bullish about bond values. If, however, a preponderance of the public becomes convinced bond prices are about to tumble, they *will* tumble because there are too few buyers at the prevailing prices. Interest rates will then rise, some capital goods will no longer be profitable to produce, and unemployment will increase. Horwitz's free banks would be able to arrest this increase in interest rates, and the unemployment it causes, only if these banks were willing to suffer losses, purchasing bonds even though their prices were being driven down by the public's self-fulfilling expectations of falling asset values.

Horwitz conceives of the interest rate as an intertemporal price that moves up and down in response to changes in the public's rate of saving. But households thinking about their future must make two decisions, not one. In addition to deciding how much of their current income to save, households must also decide how to allocate their existing wealth among alternative assets, a decision that will be driven in large measure by expectations regarding future asset prices, as well as by the degree of confidence with which these expectations are held. This asset allocation decision has a much stronger influence on the rate of interest than the decision about current saving, because the stock of existing wealth to be allocated is so much greater than the flow of new saving. At any given time, the quantity of preexisting stocks and bonds that can be released onto the market overwhelms the quantity of new stock and bond issues entering the market. Similarly, the quantity of money and near-money being held in expectation of a fall in securities prices exceeds by many times the volume of new saving. It is because the interest rate is tethered to the expectations, hence the liquidity preferences, of those who hold the massive preexisting stock of financial assets that it cannot, at the same time, balance the flows of saving and investment. There is nothing within the institution of free banking that would emancipate the interest rate from this burden.

V. SAVING, INVESTMENT, AND FINANCE

Horwitz imagines that production is directed toward consumption and investment goods in proportions determined by the public's willingness to wait, and that if inflation is to be kept at bay, banks cannot lend more than "the public is willing to save" (10). The hidden

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premise in this argument (and in many others Horwitz advances) is that income and output are given at the level of full employment. If there are no idle resources, it then follows as a matter of course that any increase in expenditure, whether financed by bank credit or otherwise, will only drive up prices, since output is already at its maximum. If, however, some resources are unemployed, then the relevant question is no longer "How much is the public willing to save *at its current level of income*?" but rather "How much saving would be forthcoming *at a higher level of (real) income*?" In an economy with complete futures markets, wherein agents could choose among alternative streams of intertemporal consumption, the equilibrium result would provide a good answer to the question, "How much is the public willing to save?" But, in the absence of such markets, it is unclear how the *de facto* level of saving even bears on the question.

In my initial rejoinder to Horwitz, I argued that an increase in investment need not await an increase in saving because "contemporary banks create credit in excess of savings" (1996, 381). In response, Horwitz claims that "competitive banks *themselves* create credit . . . only if they have excess reserves to back it up, or if the public is willing to hold balances of the bank's liabilities" (9–10, Horwitz's emphasis). Horwitz seems to have forgotten that modern banks operate under a "fractional reserve" system, so that if reserves of 10 percent are required, deposits may expand until they are 10 times as large as reserves. Moreover, "in the real world, a bank's lending is not normally constrained by the amount of excess reserves it has at any given moment" (Federal Reserve Bank of Chicago 1992, 37). Even if there are no excess reserves, banks can still accommodate an increased demand for investment loans by inducing the public to part with some of its currency in favor of interest-bearing deposits. Since only 10 percent of new deposits must be held in reserve, $100 in new deposits can support $1,000 of additional transaction deposits (ibid., 16). Horwitz may insist that it is only because the public is willing to hold $100 in new savings deposits that banks are able to add an additional $1,000 to the credit lines of borrowers, but there is no doubt that the lion's share of new investment spending from these accounts is being financed by bank credit, not by "savings proper."

When we remove the two essential premises of Horwitz's argument, i.e., full employment and lending no greater than prior savings, the following scenario comes into view: banks lend; firms borrow, invest, and repay their loans out of the increased sales revenue.[4] In part,

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this is the familiar story of the multiplier. When there are idle resources, an increase in investment spending will increase aggregate income, and out of this increased income, there will be additional saving equal to the additional investment. The finance required for this expansion cannot come from savings that do not yet exist. Hence, the expansion must be facilitated by an extension of credit. Once investment projects have been completed, firms often will repay their short-term bank loans by issuing long-term bonds, which, in turn, will absorb the *ex post* saving.

Let us now address Horwitz's objection to this process: his claim that, "without any prior voluntary saving by the public," an increase in bank lending will lead to inflation and "forced saving" (10). If, by "forced saving," Horwitz means any saving in excess of the saving that would be forthcoming at full employment, then there is no disagreement between us. But if "forced saving" is taken to mean any addition to saving that results from a rise in prices, then I must object. If total output is increased and prices rise because marginal costs are rising, it is not clear why any additional saving should be called "forced saving." No one has a natural right to buy at low prices because they are associated with low levels of aggregate output and income (Keynes 1936, 328). Where, after all, is the coercion supposed to occur? The bank is not forced to lend; the firm is not forced to borrow; and the public is not forced to divide its income in a prescribed manner between consumption and investment, nor is it compelled to allocate its wealth among money and other assets in predetermined amounts. If expectations regarding income, prices, and other variables are not satisfied, the disappointment cannot be laid solely at the feet of lenders and borrowers. Rather, it is the natural outcome of decisions taken in ignorance of one another.

VI. CHOICE, COORDINATION, AND ALTERNATIVE ECONOMIC ARRANGEMENTS

The implicit premise in many of my arguments is that economic institutions in which decisions can be made with a knowledge of their consequences are generally superior to economic institutions in which agents must choose without this knowledge. The two alternative economic arrangements I outlined in my first rejoinder to Horwitz—an auction mechanism for pooling the investment plans of large firms, and a market in which workers could insure themselves

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against falling wages—were designed to narrow the range of uncertainty facing market participants, the first by coordinating investment decisions and the second by affording workers an opportunity to choose the level of income risk they wish to bear.

The Walrasian model of general equilibrium, and its perfect coordination of individual plans, is not helpful in understanding real-world market economies because, to achieve its perfect coordination of decisions, it must abolish the uncertainty arising from decisions not yet made. Although Horwitz and I agree on this point, we disagree about whether the range of future outcomes can be narrowed by choices made in the present. Both of the proposals mentioned above assume that some market participants would find it advantageous to enter into binding agreements today in order to reduce the range of future outcomes they would have to contend with tomorrow. It is important to bear in mind, in this regard, that holding liquid assets is not the only way of coping with uncertainty; contracts also serve this purpose, and usually without the adverse social consequences engendered by a heightened demand for money.

Horwitz argues that the provision of insurance against wage reductions that are beyond the control of individual workers would require the same sort of actuarial certainty that underlies conventional home and life insurance. But if by "insurance," we mean, more broadly, contracts in which one party agrees to bear the risk of another in exchange for a premium, then Horwitz's assertion does not fit the facts. There is, in principle, no difference between a futures market in corn where farmers can transfer price risk to commodity brokers, and a futures market in wages where workers could transfer income risk to either a private firm or to society as a whole (see Shiller 1993). A firm that is willing to bet on wage increases and, hence, to pay claims in the event of wage decreases, is not unlike a commodity broker who bets on an increase in corn prices and incurs losses if prices should happen to fall. Similarly, firms that would benefit from falling wages and wished to hedge their bets could do so by accepting premiums now in exchange for a commitment to pay claims in the event of decreasing wages. All of the uncertainties Horwitz cites as obstacles to this insurance scheme—uncertainty about "new inventions," the introduction of "computer and robotics technology," the "rise of Japanese competitors" (14–15), etc.—*already* confront investors who buy and sell any asset with an extended life.

The same point can be made in response to many of the objections

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Horwitz raises against the auction scheme for coordinating investment decisions. In listing all the complexities and tacit knowledge that go into a firm's investment decisions, Horwitz seems to have forgotten that these decisions must be made in any case. Firms must already decide how much to invest without knowing how interest rates will change or how much the public plans to save. One of the main sources of uncertainty facing firms considering investment projects is the future level of aggregate demand, which, in turn, is strongly influenced by the level of aggregate investment. Providing a framework within which the investment plans of large firms could be coordinated with respect to aggregate expenditure, if not with regard to composition, *is* a "discovery procedure" (14) that would generate useful information about aggregate spending.

In concluding, I want to return to my original point: in real-world capitalist economies, incomes are determined in significant measure by a game of chance. Without markets in which plans can be coordinated before final commitments are made, individual choices will produce outcomes that are inefficient and unfair. This result would not be improved upon by the establishment of free banking, an institutional arrangement that is ill designed for the solution of collective action problems. Macro insurance markets are better suited to the twin purposes of coordinating investment plans and affording market participants greater opportunity to insure themselves against outcomes that are beyond their individual control.

NOTES

1. For simplicity, I assume that households are net savers and firms are net borrowers.
2. This point is developed in Clower 1969 and in Leijonhufvud 1968.
3. See, for example, Stiglitz 1994.
4. For present purposes, I assume that the forecasts of the borrowing firms are validated.

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Daniel M. Hausman

THE FAULTS OF FORMALISM AND THE MAGIC OF MARKETS

ABSTRACT: *Contrary to Peter J. Boettke's essay, "What Went Wrong with Economics?", there is no connection between "formalism" and the alleged inability of mainstream economists to regard theoretical models as anything other than either depictions of real market economies or bases for criticizing market economies and justifying government intervention. Although Boettke's criticisms of the excesses of formalism are justified, Austrian economists such as Boettke need to justify their view that government interventions into economic affairs are inevitably harmful.*

In his essay, "What Went Wrong with Economics? Equilibrium as a Flight from Reality" (*Critical Review* 11, no. 1), Peter Boettke offers the reader a historical diagnosis of the ills of economics. The root problem is methodological. Instead of treating the notion of an equilibrium as an ideal type, to be used as a point of contrast for the study of economic institutions and processes, economists have either pretended (incredibly) that real economies are in equilibrium, or they have maintained that departures from equilibrium represent economic failures to be rectified by government intervention. Boettke

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Daniel M. Hausman, Department of Philosophy, University of Wisconsin-Madison, Madison, WI 53706, is the author of *Capital Profits and Prices: An Essay in the Philosophy of Economics* (Columbia, 1981), *The Inexact and Separate Science of Economics* (Cambridge, 1992), *Essays in Philosophy and Economic Methodology* (Cambridge, 1992), *Economic Analysis and Moral Philosophy* (with Michael McPherson, Cambridge, 1996), and *Causal Asymmetries* (Cambridge, 1998).

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maintains that this central methodological mistake is tied to two others. First, economists have become so enchanted with their mathematical tools that they have become unable to make use of any claims about economics, no matter how insightful, unless they can be formulated within a mathematical model. Second (and largely as a consequence), economists have had almost nothing to say about disequilibrium and dynamics. So instead of developing the sort of rigorous "realistic" theorizing exemplified in Hayek's work—which concerns the contrasts between static equilibrium and real, dynamic disequilibrium dynamic processes—economists have lost themselves in their formal models, rarely looking out at the real world and then only to pretend that their model *is* the real world; or, alternatively, in preaching government intervention to make the real world conform to the model. The result is just the disastrous interventionism that Hayek feared, though he thought that it would come from the anti-theoretic stance of historicists rather than from the excesses of "formalism."

Although Boettke diagnoses some of the deficiencies of contemporary economic theorizing, the details of his criticisms are hard to pin down and to substantiate. In particular, Boettke's critique of the misuse of the notion of competitive equilibrium is puzzling, and its connection to his condemnation of formalism is hard to understand. The alternative to formalism supposedly exemplified in Hayek's economics is never spelled out, and when one looks at the contrasts Boettke draws between Hayek's work and the work of a contemporary "formalist" such as Stiglitz, the puzzle only grows.

Adam Smith suggested that agents pursuing their own interests in market economies will (as if directed by an invisible hand) serve the common good more effectively than if they aimed directly at the common good. Smith could make no rigorous argument for this conjecture, but the notion that a decentralized economy might promote overall welfare seemed to conform to the experience of Smith's successors, and his conjecture captured their imagination. In this century economists have apparently proven a version of Smith's claims about an invisible hand. *If* individuals are perfectly informed and in a specific sense rational and self-interested, *if* production possibilities are well-behaved, *if* there are many buyers and sellers in every market and markets for every (perfectly divisible) commodity at every date and place, and *if* there are no externalities or public goods, then at least one Pareto-optimal economic equilibrium obtains. The thought that decentralized markets "work" is captured in the conclusion that there

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exists an economic equilibrium—that is, is a state of affairs in which there are no excess demands in any market and no excess supplies of any good that is not free. The thought that people are brought to serve the common good is translated into the conclusion that this equilibrium is Pareto optimal—that is, that there is no alternative that is unanimously preferred to it.

One might well wonder why economists are so excited about the notion of a Pareto optimum, since it is not very high praise for a social state S that for any alternative S', somebody prefers S to S'. Moreover, even if one supposes that it is a good thing to satisfy people's preferences and denies that interpersonal comparisons of the extent to which preferences are satisfied can be made, one cannot infer that R is better than Q from the fact that R is Pareto optimal and that Q is not. The proof that competitive equilibria are Pareto optimal is better interpreted as showing that competitive equilibria do not show a particular vice—that they do not pass up unanimously favored improvements—rather than that they possess any particular virtue. Yet at least this much can be proven, and the proof has played an important role in arguments concerning the welfare properties of markets. Together with the so-called second welfare theorem (that any optimal outcome can be achieved as a competitive equilibrium given the right initial distribution of endowments), this result lies at the foundation of modern welfare economics.

Yet the proof of the existence and optimality of competitive equilibrium does *not* establish Smith's conjecture. His conjecture concerned the outcome in real societies of real self-interested behavior, while the theorem concerns the hypothetical outcome of perfectly rational self-interested behavior in circumstances that are strikingly unlike any actual circumstances. What possible point could there be in theorizing about a Never-Never Land of perfect knowledge, complete futures markets, infinite divisibility, non-increasing returns to scale, and so forth? One answer is that reality approximates this Never-Never Land in its economically relevant respects. Like Boettke, I do not think this response is tenable, and I shall say nothing more about it. A second answer is that one might wonder whether a Pareto-optimal competitive equilibrium *could* be reached even if we were free of all those complexities that distinguish our world from Never-Never Land. Obviously not everyone would or should be interested in this purely theoretical question, but given the prevalence

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of economic theorizing employing such models, one can argue that there is some value in asking this odd "What if" question.

Be that as it may, Smith's conjecture has not been proven. Historical experience has established that market economies that have been subjected to relatively little government interference have grown more rapidly and persistently than other kinds of economies that have been tried thus far. That experience does not, however, establish that market economies are better than untried alternatives; and only in exceptional circumstances does the historical record warrant any view of what the consequences would be of any particular proposal for reforming market economies. To address such questions, one needs economic theory. Boettke complains that contemporary "formalist" economic theory is unable to answer such questions. Because of their insistence on mathematical modeling, formalist economists can only deal with unrealistic non-existent circumstances, and they are consequently forced either to pretend that reality approximates their simplistic models or that reality could be made to approximate the models with the help of judicious government intervention. But we are not in perfectly competitive equilibrium, and government intervention could not possibly overcome all the barriers that forever separate us from perfectly competitive equilibrium.

In that case, what possible use could models of perfectly competitive equilibrium have? One answer is that these models are of no use at all. For example, Joseph Stiglitz (1994, 267) writes,

> Imperfect and costly information, imperfect capital markets, imperfect competition: These are the realities of market economies—aspects that must be taken into account by those countries embarking on the choice of an economic system. The fact that competition is imperfect or capital markets are imperfect does not mean that the market system should not be adopted. What it does mean is that in their choices, they should not be confused by theorems and ideologies based on an irrelevant model of the market economy. Most important, it means that in deciding on what form of market economy they might adopt, including what role the government ought to play, they need to have in mind how actual market economies function, not the quite irrelevant paradigm of perfect competition.

This radical answer is not Boettke's or Hayek's. Boettke (1997, 23) argues that models of perfectly competitive equilibrium should be regarded as *ideal types*.

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> An ideal type is neither intended to describe reality nor to indict it. It is instead a theoretical construct intended to illuminate certain things that *might* occur in reality; empirical investigation determines whether these phenomena are actually present and how they came to be there.
>
> Deployed as an ideal type, equilibrium analysis allowed economists to describe what the world would be like in the absence of imperfections such as uncertainty and change. The descriptive value of the model lay precisely in its departure from observed reality, for this underscored the function of real-world institutions in dealing with *imperfect* knowledge, uncertainty, and so forth.

Although there are some obscurities in Boettke's account (in what sense "might" there be complete futures markets?), the basic idea seems clear enough. The point of an ideal type is to provide a perspective or a point of comparison. By studying how reality differs from the ideal type, economists can learn things about actual markets that they might otherwise have overlooked. Ideal types appear to have an entirely heuristic role. A "good" ideal type is one that helps economists to learn about economies, while a bad one does not. The extent to which the ideal type approximates reality is not itself of any particular importance.

If this interpretation of Boettke's view is correct, then it is puzzling why the model of competitive equilibrium should be so prominent. The reason would have to be that comparing actual economies with an ideal type of a perfectly competitive equilibrium is in fact more informative than comparing actual economies with an ideal type of a command economy, a Keynesian economy, or any of a dozen different institutionalist ideal types. This seems dubious. For heuristic purposes, one would have thought eclecticism desirable. Many different perspectives should be more informative than just one.

Boettke himself (1997, 44) seems ambivalent about whether ideal types are only of heuristic value:

> The main task of any science is to investigate the degree of correspondence between various ideal types and empirical reality; but this means that science is primarily a matter of experimentation or, in social science, historical research—not model-building but model-testing (i. e., testing of the applicability of intelligible models to given situations). Still, the "falsification" of an ideal type in a given instance does not require that it be discarded as useless. It may aid the scientist in con-

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> structing ever more realistic models of the circumstances of that particular time and place. . . .
>
> Treating models as ideal types allows one, or one's colleagues, to root out one's ideological prejudices by subjecting one's models to the empirical test of applicability as well as the philosophical test of intelligibility.

When Boettke boldly asserts that "the main task of science is to investigate the degree of correspondence between various ideal types and empirical reality," presumably he does not mean that scientific theories have only a heuristic significance. It would be odd to speak, as Boettke does, of "testing" theories or models, if the role of theories and models were only to guide the study of reality. In addition to their heuristic role, it seems that ideal types also aim to represent reality. The heuristic role only becomes central when the representational effort fails.

Although this view makes better sense of Boettke's text, we still have not explained why variations on one particular ideal type should possess such a hegemony in economics. When one turns to Hayek's own work for an answer, one finds the following remark:

> While in the field of the Pure Logic of Choice our analysis can be made exhaustive, that is, while we can here develop a formal apparatus which covers all conceivable situations, the supplementary hypotheses must of necessity be selective, that is, we must select from the infinite variety of possible situations such ideal types as for some reason we regard as specially relevant to conditions in the real world. (1949, 47)

Hayek is here distinguishing between two components of economic models. One part consists of what Hayek calls "the Pure Logic of Choice," by which he means some formulation of (at least some subset of) the fundamental principles of contemporary economics. The model of individuals as rationally pursuing their own interests is not, in Hayek's view, an ideal type, because this "formal apparatus" "covers all conceivable situations." Economists only generate ideal types when they add simplifying assumptions concerning, for example, knowledge or the absence of externalities. Hayek believes that his own concern with an ideal type of competitive equilibrium is justified by the tendency of real economies toward equilibrium (1949, 44–45, 49). It is hard to say exactly what this means and exactly how the equilib-

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rium he is thinking of relates to perfectly competitive equilibrium, because the factors that prevent an economy from reaching equilibrium are so deep and pervasive. But the importance of equilibrium for Hayek is not mainly heuristic. What explains and justifies the ubiquity of equilibrium models in "formalist" economics (most of which are *not* models of perfect competition) seems to be that these are the only mathematically tractable models that embed the "Pure Logic of Choice." One finds not just one ideal type, but a whole range that share a common core that is not itself (in Hayek's view) an ideal type.

Boettke is right to complain that the contemporary range of these ideal types is limited by the insistence on formalization, but his charge that formalist economists in general misunderstand the nature of their constructions is hard to substantiate. Of course there are individual formalist economists (like individual Austrian economists or individual stamp collectors) who have said some very silly things. The question is whether formalists in general overlook the heuristic uses of ideal types. Notice that there is no *methodological* mistake—though there may be empirical error—in arguing that a particular ideal type approximates reality or that with appropriate government intervention, such an ideal type would approximate reality. The methodological mistake Boettke identifies is supposed to lie in an inability to understand how an ideal type could otherwise be of value.

> Absent from both types of formalism was recognition of any possibility other than all or nothing. Either the real world exemplified static equilibrium, or it could not approach that state without a push from the state. The intermediate possibilities represented by real-world institutions of adjustment to disequilibrium became invisible because the model contained only equilibrium. (1997, 19)

The earlier quotation from Stiglitz might be taken as exemplifying this mistake. Since Stiglitz is acutely aware that economies are not in perfectly competitive equilibrium and cannot be brought into it via government intervention, he asserts that "the paradigm of perfect competition" is "quite irrelevant."

Without undertaking an extensive literature review, I cannot determine whether "formalist" economists in general commit this sin, but it seems fair to focus on Stiglitz's work, since Boettke singles Stiglitz out as exemplifying the blunders of formalist economics. If Stiglitz

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does not make the mistakes that Boettke alleges, it might be that Boettke is wrong merely about Stiglitz, who turns out to be an exception to the erroneous rule. But if Boettke's own evidence conflicts with his claim, one can conclude that his claim is at least unproven.

In the extracted quotation, Stiglitz says that the model of perfect competition is "irrelevant" for *policy makers*. He does not say that it is irrelevant for theorists. Unless Stiglitz is prepared to condemn most of his own life's work, which has been devoted to studying ways in which economic reality differs from perfect competition, he could not possibly believe that the model of perfect competition is of no importance for economic theorists. He asks, for example, whether imperfect knowledge is consistent with the absence of externalities (1994, 29–30)—which would not be worth asking if one did not "take seriously" a model in which externalities are absent. He argues that a complete set of markets is inconsistent with perfect information and indeed is itself a conceptual impossibility (1994, 37–9), and the reason he thinks about the possibility of complete markets is that perfectly competitive equilibrium requires them. So Stiglitz does not believe that the model of perfectly competitive equilibrium is worthless.

Nor does Stiglitz maintain that reality is approximately in perfectly competitive equilibrium or that government intervention could bring this about. Having argued at great length that markets are necessarily imperfect, the question cannot be how to make them perfect. The question instead is what set of institutions will bring about the best results—and there is absolutely no presumption that government intervention will improve matters. To the contrary, Stiglitz addresses section after section of his 1994 book to a painstaking account of the risks and difficulties to which government engagement in economic life is subject. In his own words:

> This pervasiveness of failures [as compared to competitive equilibrium], while it reduces our confidence in the efficiency of market solutions, also reduces our confidence in the ability of the government to correct them. (1994, 44).

Boettke's accusation cannot, therefore, be sustained. Stiglitz does not regard perfectly competitive equilibrium as an ideal that can be realized with the help of government intervention. On the contrary, Stiglitz employs the model of perfectly competitive equilibrium pre-

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cisely as an ideal type: He uses the comparison between reality and the model to identify institutions and practices that are important because of the divergence between reality and the model.

> The advocates of market socialism made, however, a critical mistake, in not paying sufficient attention to the institutions (e.g., banks) which have arisen under capitalism to solve the problems created by absent markets. They are not a perfect substitute. But they may result in an allocation of investment that is better than it otherwise would be—and better than it is under market socialism. (Ibid., 92)

What is it, then, that bothers Boettke so much about Stiglitz's work? Why does he place Stiglitz in the camp of those who reject "empirically meaningful and rigorous theorizing" (Boettke 1997, 46) rather than welcoming him to the side of the angels? One reason is that Stiglitz is a formalist: He values mathematical modeling, and he probably believes that if an idea "could not be translated into an appropriate model, there was not much that could be done with it" (ibid., 22). But as Stiglitz's work demonstrates, such a commitment to "formalism" is fully consistent with regarding economic models as ideal types. The commitment to mathematical modeling does not imply (and indeed has little connection to) the view that models must be regarded as approximating reality or as ideals to be achieved by means of government intervention.

Is formalism of the sort exemplified by Stiglitz's work a mistake? The question is ambiguous. It might be interpreted as: "Is it sometimes a mistake to address economic questions with mathematical tools?" or as "Is it a mistake to address them *only* with mathematical tools?" Boettke would, I believe, answer yes to both of these questions, but let us address them separately. Consider the first question first. Boettke holds that it is sometimes a mistake to attempt to address economic questions with mathematical models, because economists do not possess the mathematical tools needed to model complicated dynamic relations. Attempting to employ mathematical models despite these inadequacies distorts inquiries concerning the consequences of ignorance or concerning processes of discovery, and it creates misunderstandings.

This is a serious and important criticism, but there are two reasons to hesitate before accepting it. First, mathematical techniques are constantly changing and improving. Explicitly dynamic problems are

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hard to model mathematically, but I know of no proof that the job *cannot* be done. The second reason to hesitate before accepting Boettke's condemnation of mathematical modeling is that one needs to consider how *serious* the distortion and misunderstanding will be and how gravely they will compromise the results. Even if formalist treatments of problems concerning knowledge will necessarily be imperfect, they will not necessarily be worthless. A wrench is the wrong tool for driving a nail, but if one does not have a hammer, it may be the best tool to use all the same. Stiglitz himself is well aware that a great deal is left out of his models. "No simple model can capture the processes by which institutions adapt to changing circumstances," he writes (1994, 25; see also 65).

Whether or not there is any mistake in addressing questions concerning ignorance, innovation, and discovery by means of mathematical models, one might agree with Boettke that it is a mistake to insist that there is no other way to address these questions. As Boettke sensibly points out (1997, 50), the syntactic clarity and precision of mathematical models do not insure their semantic clarity or precision; and indeed confusion concerning the interpretation of theorems is very common in economics. I am personally quite sympathetic to Boettke's point here. Many economists have become so enchanted with mathematical techniques that they are unable to appreciate important observations and valid arguments when these are made in ordinary prose.

But there is a deeper issue. At one point Hayek observes that even when competition is imperfect "this does not mean the competition does not also bring about as effective a use of resources as can be brought about by any known means where in the nature of the case it must be imperfect" (1949, 104). Quite so, but does competition in fact accomplish this? Does it "bring about as effective a use of resources as can be brought about by any known means?" Boettke writes (1997, 27), "Of course, in this process of perceiving the future, entrepreneurs may (and do) make errors, but these errors can, by creating further discovery opportunities, generate further activity aimed at allocating or reallocating resources in a more effective manner to obtains the ends sought after." This *seems* possible. But is it? What, in fact, are the consequences when entrepreneurs seize the opportunities created by the errors of others? Does their activity improve or diminish economic performance?

How are such questions to be addressed without mathematical

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modeling? The theory of the second best (Lipsey and Lancaster 1956–57) removes any presumption that corrections of errors necessarily improve outcomes. Without such a presumption, and without mathematical modelling, how could Hayek's or Boettke's claims be established or refuted? Stiglitz asks, "If markets do not work efficiently under these idealized circumstances, how can we be confident that they would work efficiently under more complicated circumstances? Only by an act of (and indeed a leap of) faith!" (1994, 26).

Boettke never responds to this critique. He is acutely aware of the need for evidence only when he is considering the views of those he disagrees with. The notion of an efficiency wage is, he maintains, "an ad hoc, empirically ungrounded dogma" (1997, 42). Boettke complains that Bardhan and Roemer's attribution of the political failures of contemporary governments to inequalities of influence owing to inequalities of wealth "is more assumed than proved" (ibid., 36). But Boettke has no such doubts about the benefits of unimpeded markets: "To find such an error is to produce economic information that is useful in a world that does *not* approximate equilibrium; to use this information to make a profit is to move that world a little closer to the normative ideal" (ibid., 33). He is not worried about the fact that this view appears to be "more assumed than proved." Boettke maintains that "the divergence between ideal and reality can highlight the ways reality may have institutionalized error-correcting properties that can, in fact, be seen as propelling the world in a direction reminiscent of general equilibrium" (ibid., 43–4). How does he know that the divergence will not highlight deficiencies in real markets? Might not his optimistic view be "empirically ungrounded dogma"?

Boettke's apparent presumption that any respectable economic theory must demonstrate the superiority of unimpeded markets to any feasible alternative may explain his dissatisfaction with Stiglitz's work and "formalism" in general much better than any of the methodological points canvassed above. Boettke (1997, 35) writes:

> Still missing from the [Stiglitz's] analysis is an examination of how imperfect human beings attempt to cope in a real world of ignorance and uncertainty. As a result, we get the bifurcation of the world into a private sector that [is] unable to do anything to cope with the slightest departure from general equilibrium, and a public sector that, being the creation of the normative equilibrium theorists' imagination, is able to rectify the resulting problem "as if by an invisible hand." As in the ear-

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lier Neo-Keynesian synthesis of Paul Samuelson, the gap between norm and reality is closed by the omniscient state.

As demonstrated by the passages quoted before, which were taken from a work of Stiglitz's that Boettke cites, this accusation is off the mark. So is Boettke's view that the majority of economists regard the market as "a dystopia, devoid of dynamic adjustment properties" (1997, 19). Although he is concerned to study the institutions that "solve the problems created by absent markets," and although he notes that they "may result in an allocation of investment that is better . . . than it is under market socialism," Stiglitz (1994, 92), like most economists, does not rule out the possibility that specific government interventions may improve economic performance. Is that perhaps his cardinal sin and indeed the fatal flaw in the discipline as a whole? Despite his insights concerning the limitations of mathematical modeling, it seems that Boettke's central concern is that economists return to their true calling as uncompromising defenders of *laissez-faire*.

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Robert Heilbroner

THE SELF-DECEPTION OF ECONOMICS

ABSTRACT: *Formalization has led contemporary economics to turn its back on the study of capitalism as the social order to which it owes its origins and its intrinsic analytical focus. As a consequence, contemporary economics turns a blind eye to the empirical analysis of capital accumulation, the real-world properties of markets, and the bifurcation of political power that endow capitalism with its unique historical properties. Instead, economics takes scientific, not social, analysis as its model, a view that gives an ideological cast to its findings.*

Peter Boettke's brilliant article (1997) sets the stage. He has given many examples of the constraints that economics has placed upon itself by virtue of its identification of mathematical formalization with empirical analysis. I shall try to depict these consequences from the viewpoint of what Joseph Schumpeter (1954, 41) called the vision—the pre-analytic cognitive act—of economics itself. In the end, the consequences are the same: formalization throws out the baby with the bath water. From the viewpoint in which I am interested, the effect is the virtual disappearance of any substantive conception of what economics "is."

Let me begin with a curious fact that has, so far as I know, entirely

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Robert Heilbroner, Department of Economics, Graduate Faculty, New School for Social Research, 55 Fifth Avenue, New York, NY 10003, telephone (212) 229-5717, most recently the author of *Teachings from the Worldly Philosophy* (Norton, 1996), thanks William Milberg and Jeffrey Friedman for many helpful comments.

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escaped the notice of the practitioners of modern-day economic analysis: the sedulous avoidance of the word "capitalism" to identify the social entity to which economics directs its attention.[1] In its place, the profession uses "society" or "system" or "market system" to describe the object of its attention—a usage that not only quietly announces the presumably apolitical nature of its field of inquiry, but which is demonstrably false. Let me attend to its falsity before speculating as to the reason for its hegemony.

What Capitalism Isn't

As it is conventionally used in economic exposition, the word "society" refers to the organization of production and distribution necessary for survival. Is this organized activity accurately captured in the clarifying gaze of economics? As I have on many occasions, I shall answer this question by taking a concrete instance from the anthropologist Elizabeth Marshall Thomas's remarkable book, *The Harmless People*, in which she describes how a group of Kalahari bushmen carry out the division of their prey. In Thomas's words, "Gai [the hunter] owned two hind legs and a front leg, Tsetchwe had meat from the back, Ukwane had the other front leg, his wife had one of the feet and the stomach . . ." (Thomas 1958, 59).

Such a description could, with variations, be repeated for the contemporary inhabitants of the jungles of South America and the Inuit peoples of northern Alaska, the Indians of precolonial America, and, for some 200,000 years, for the earliest known life of *Homo sapiens*. My question is: Can economics—Classical, Neoclassical, Austrian, Marxian, or formalistic—explain the "economics" behind these distributive acts? More concretely, could any economist foretell, before witnessing the distribution of the kill, who would get what; or explain, after the fact, why who got what? My answer is negative, because the explanatory principle involved is alien to economics as we know it. The principle is kinship—the detailed network of familial relationships that determines the actors' reciprocal social obligations. Economists may declare that the participants were maximizing their utilities, pursuing rational choice, or whatever, but I would maintain with assurance that without the missing concept of kinship, these explanations could convey nothing. Thus economics, as we understand the term, does not apply to societies in the hunting and gathering

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forms in which humanity has lived for over 98 percent of its time on earth.

Let us now move to the remaining three to four thousand years of early history. How useful is "economics," as we know it, in explaining production and distribution in the stratified societies that emerge along the Euphrates, the Nile, the Pacific coast of South America, the Yangtse, or, for that matter, Russia during its Stalinist years? Could professors of economics enlighten us about the rationales behind the construction of the massive temples, city walls, pyramids, or industrial complexes (to bring Russia into the picture) that gave these stratified systems their distinctive organizational characteristics?

As before, my answer is No. To understand the Sumerian, Akkadian, Egyptian, Incan, and Soviet systems of production and distribution does not require the special explanatory insights of economists. Engineering problems, managerial problems, military considerations, yes. Economics? No. Put differently, just as production and distribution in hunting and gathering societies must be explained by a knowledge of their traditions, stratified societies can be "explained"—that is, anticipated before the fact or rationalized thereafter—only by knowledge of their structures of command. Economics had or has nothing to say about Cheops' pyramids, China's Great Wall, or the Soviet industrial structure: the will of pharoahs and emperors and the Communist dictatorship said it all.

Then when does economics enter? The answer, of course, is with the advent of capitalism, a lengthy process that takes on recognizable form around the beginning of the eighteenth century. Let me make the point by quoting from the work of a remarkable English trader in France, Richard Cantillon, whose pathbreaking *Essay on the Nature of Commerce*, published (in French) in 1730, describes the emerging economy in a small French village square where meat vendors confront housewives:

> Suppose the Butchers on one side and the Buyers on the other. The price of Meat will be settled after some altercations, and a pound of Beef will be in value to a piece of silver pretty nearly as the whole Beef offered for sale in the Market to all the silver brought there to buy Beef.
>
> This proportion is come at by bargaining. The Butcher keeps up his Price according to the number of Buyers he sees; the Buyers, on their side, offer less according as they think the Butchers will have less sale;

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> the Price set by some is usually followed by others. . . . Though this method of fixing prices has no exact or geometrical foundation, . . . it does not seem that it could be done in any more convenient way. (Cantillon [1755] 1964, 117, 119)

Clearly this is a society in which production and distribution can be explained neither by Tradition nor Command. It is a society run by "economics," although that word is not yet known, and its designation is "capitalism," although that term will not enter the conventional vocabulary until the late nineteenth century. By way of passing interest, neither "capitalism" nor "economics" appears in the *Wealth of Nations*.

What Capitalism Is

It remains to make explicit the institutional characteristics of such a society. They are three; and although all are familiar, it is part of the general deficiency of modern-day economics that important aspects of each are virtually never spelled out.

The first of the three is *acquisitiveness*, both as a general characteristic of everyday "economic" life, and in the more structured form of capital accumulation. Both forms have today acquired more respectable names: everyday acquisitiveness has become maximization; capital accumulation, investment. That which interests us here is the neutralization of these once charged terms: the *chrematistiké* (acquisitiveness) that Aristotle considered to be a character flaw has attained respectability; the relentless drive to increase capital that Marx saw as the Achilles heel of the system has become the royal road to growth.

I mention these seemingly trifling matters of vocabulary because they concern aspects of behavior that cannot be reconciled with the assumptions of a formalized maximizing economic model. As we shall note, Aristotelian, Marxian, Veblenian, Freudian, not to mention Biblical, emphases on the moral and psychological wariness with which money-making has long been held may contribute to our "vision"—our pre-analytic grasp—of the behavior of agents in a capitalist economy, but they have no place in the depoliticized, depsychologized, and desociologized depiction of such behavior that forms the basis of the formal models Boettke criticizes. In a word, the energy that provides the vital force of a capitalist order is presented as a for-

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mal property of the larger system, without psychological origins, social influences, or political significance. It is "behavior" stripped of everything that is behavioral.[2]

I turn next to the second characteristic element of capitalism. It is, of course, *the market*, the most important of the elements that lend themselves to formal analysis. The fascination with markets is entirely understandable and justified—they are unique institutional/behavioral mechanisms that endow society with a spontaneous self-corrective capability of astonishing properties: we recall Cantillon's use of "exact" and "geometric" in describing the outcome of the confrontation of the Butchers and the Buyers.

What is missing in this imaginative and insightful representation? Samuel Bowles and Herbert Gintis attempt to throw some light on the matter in an essay otherwise very much in general accord with my own view. They write: "We have deliberately attempted to model the capitalist economy rather than economies in general. . . . It is well known that markets are allocation mechanisms . . . but they are also disciplinary mechanisms" that "determine and shift the production possibility frontier" (1990, 203). But if the word "economy" is to have any relevance to their and my inquiry, what light is thrown by contrasting the operations of a "mechanism" that operates only within the sociopolitical ambit of capitalism with its operation in "economies in general," which, as we have seen, do not exist? We are delighted when Cantillon sees that markets are not merely the scenes of petty altercations, but the loci of an order-bestowing process, and we would be disconcerted if Bowles and Gintis did not make that understanding central to their analysis. Why, then, do even such politically sensitive economists pass over, rather than drive home, the specific and exclusive relevance of markets, whether as allocational or disciplinary "mechanisms," to the social formation called capitalism?

This brings us to a second element in market behavior that also escapes the general notice of economists who depict the market as an interaction among "factors of production" viewed as economic equals. Exchange among marketers is thereby made to seem utterly unlike that between the feudal lord and his peasants and slaves, in that it is determined by the free choice of its participants, motivated solely by a desire to "maximize" their well-being. I would be the last one to deny the gulf that separates the workings of a market society from one run by command, but one needs also to bear in mind the continuance of inequality, albeit on a much smaller and less cruel scale, that

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makes markets "disciplinary" as well as "allocative" mechanisms, to repeat the description by Bowles and Gintis (who are, I hasten to add, thereby excused from this generalized oversight).

Not surprisingly, the market's disciplinary function is particularly applicable to relations between workers and employers. Both own their own means of production—laboring power in one case, land or capital in the other. Neither can be forced to supply their means of production if they are unwilling to do so. But whereas the worker is compensated only to the extent of his market wage, the employer retains ownership of the product of his employee's labor, and can claim as his own whatever that product yields over its cost. This asymmetrical distribution of product reflects the presence of social disparity, however unrecognized. The market's determinations can, of course, be resisted, by the exercise of a counter-power, as when workers mount an effective strike. In general, however, the market relies, as do all stratified systems, on what Adam Smith (1776, 532, 670) calls the "habit of subordination"—itself assuredly a sign of inequality.

I move finally to the third element of capitalism—its bifurcation of power between the "public" and the "private" sectors. The sectors are differentiated in many ways, but the most striking is the allocation of direct and indirect power. The role of the private sector is essentially one of indirect power—let us call it "domination"—that exercises its influence over the institutions and purposes that establish the economic agenda of the larger society, in particular its "business" goals and values. Note, however, that this does not grant the private sector rulership. Capitalists dissatisfied with the performance of their workers may fire them, but cannot imprison or otherwise harm them; business may lobby for legislation favorable to its interests, but cannot enact it.

The public sector does indeed rule, in the sense of possessing the right to punish, to establish laws, to declare war—all within the bounds of democratic constraints that did not exist to inhibit rulership in precapitalist times. Nonetheless, this exercise of rulership is on the whole easily accommodated to the domination of the private sector. Needless to say, conflicts of interest and interpretation are common, as well as tensions between the strategies of the business community and those of the political leadership, but these conflicts do not threaten the legitimacy of the system, although they may on occasion endanger its effectiveness.

Insofar as there exists no frank and self-assured political discourse

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within capitalism, comparable to that which enabled aristocratic orders to speak openly of their rights and privileges, one sees little explicit discussion of these issues in the professional economic literature. At best, there is a recognition of business "influence," and of the struggle within the system between business interests and those of the regulative function of government. But at a deeper level, the political nature of capitalism is obscured in a kind of studied denial. The bifurcation of power which is indeed a central feature of capitalism thus becomes a means of obfuscating the hierarchical nature of the system as a whole.

One important point must be added to this defining triad of capitalist chracteristics. It is a recognition that variants in these three essential structures give rise to significant variations in the operations of different national capitalisms. One has only to reflect on the social textures and operational performances of the Japanese, Norwegian, Italian, Brazilian, and U.S. economies to recognize that there is a spectrum of capitalisms, all recognizable by their similar social architectures, but each distinguished by the intensity of its acquisitive drive, by the formal and informal operations of its market system, and, perhaps of greatest significance, by the relations between the two sectors. The structure of capitalism, as I have outlined it, is therefore an ideal type capable of encompassing societies whose workings display considerable differences. Not surprisingly, the existence of such a spectrum of capitalisms attracts little attention from the economics profession. Absorbed in the formal properties of equilibria of various kinds, economists have little time to waste on such "cultural" questions as why Dutch capitalism is tangibly different from French, or whether the exploration of such questions might not possibly lead to a better understanding of the successes and failures of capitalism in the United States.

What Went Wrong with Economics?

What lies behind the lack of interest of economists in examining, much less speaking frankly of, the sociopolitical architecture of capitalism? Some light is surely thrown on the matter if we reflect on the long historical ambivalence surrounding acquisitiveness in general, and the more recent reluctance of democratic societies to speak openly of class structure. However, I join Boettke in ascribing much

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of the responsibility for the increasingly myopic vision of the economics profession to its infatuation with science.

As the most esteemed (and sometimes feared) human activity, science has today taken the place once occupied by religion in allaying what Adam Smith (1980, 45–46) called "the tumult of the imagination." The voice of science thus becomes that desired by Economics, in contrast to the transparent values and preferences that make the second word in "Political Science" a mere honorific. Yet even the most distinguished economists freely violate this presumed neutrality of discourse.

A few egregious examples, cited in the end notes,[3] will make the point, but the deeper issue is not revealed in embarrassing revelations that betray a speaker's undeclared value preferences. It lies in the revelation of those preferences in presumably value-free statements, such as those made by economists in the name of a value-neutral "scientific" viewpoint. This brings us to the delicate subject of ideology, to which the remainder of this paper will be mainly devoted.

Here I think it useful to return to the distinction between Vision and Analysis. Schumpeter himself feared that the ideological content of vision might knowingly bias the analytic inquiry on which economic theory was based (1953, 42). Unlike Schumpeter, I believe that ideological considerations express unconsciously held value judgments, rather than deliberate distortions of what the speaker knows to be the "truth." As such, ideological views are discoverable in many kinds of pre-analytic visions, from social stereotypes to blanket institutional endorsements or denunciations. Such distortions can be avoided only to the degree that we become aware of a bias in our vision, and decide it to be untrustworthy or mistaken—an experience common in matters of small personal account, but much more unusual when the issue challenges our broad social views.

Thus, I do not see ideology as referring to the propagation of conscious values, as when a citizen of a democratic country argues for the "self-evident" virtues of open elections. In sharp contrast, I see ideology in assertions with respect to whose sociopolitical content the speaker is quite unaware. As the reader already knows, this applies with a vengeance to the identification by economists of the object of their study as the behavior of "society," leaving unmentioned the stratified character of the social order to which they direct their gaze. In the same fashion, I deem modern economics to be ideological when it describes its analytic procedures as "dispassionate" (Mankiw

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1998, 18) or as embodying a "scientific view" (Stiglitz 1997, 18), again ignoring the value judgments implicit in its arguments.

Here I content myself with a single but strategically important example regarding the relative social usefulness of public versus private borrowing and spending. Only to give specificity to a nearly universally held view, I shall again cite the words of two highly regarded economists, Gregory Mankiw and Joseph Stiglitz. After what is clearly meant to be a balanced discussion of both sides of the issue, Mankiw concludes (1998, 557, his italics): "When *the government reduces national saving by running a budget deficit, the interest rate rises, and investment falls.* Because investment is important for long-run economic growth, government budget deficits reduce the economy's growth rate." Similarly, Stiglitz (1997, 309) sums up his own discussion with: "Reducing the deficit . . . allows interest rates to fall, stimulating investment, and thus promoting growth and better future living standards."

Why are these ideological statements? Because both depend on the unproved assumption that government spending will be less productive of growth than private spending. Will it? The answer obviously depends on what the two sectors do with their borrowed funds. Thus, suppose the government's spending was designed to strengthen the nation's education structure and to finance housing for the nation's poor, whereas the private investment was intended to create a new chain of Disneylands and gambling casinos. Is it not extremely probable that such a program of public borrowing and spending would bring more future benefit than that of the private sector?

My example is deliberately made ridiculously one-sided. One has only to substitute egregiously wasteful government projects and beneficial private ones to reverse the verdict. But is that not the very point? Does not private investment win the economists' vote because it is *assumed* that public investment will be less beneficial than that of the private sector—with one enlightening exception? The exception is the case of war, when all agree that government spending for military purposes must have priority over private expenditure. Aside from that, there is no "scientific" reason to award priority to either sector on the undemonstrated assumption that its projects will be more beneficial than those of the other. But in the specific quotations cited above—citations whose conclusions will be found in all major economic texts of which I know—private expenditure, especially for in-

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vestment, is deemed to be more beneficial than its public counterpart, without a shred of empirical evidence.

I need hardly add that there are many difficulties in devising convincing comparisons of the effects—especially the welfare effects—of public and private spending. Social and political considerations aside, there are genuine statistical and other problems in making such judgments, both within each sector and between them. But difficulties are one thing; blanket determinations another. The first leads to a relatively "scientific" resolution of the problems of ranking the projects best suited to the future; the second to an ideological decision in favor of private spending, especially for capital accumulation, because that is the ranking favored by capitalism.

Setting Economics Right

Here we return to the central defect of economic thought, as I see it: its reluctance to admit its exclusive relevance to the sociopolitical formation whence it springs. Why this reluctance? I have already mentioned the difficulty of speaking frankly about power in a system in which stratification—that is, class structure—is unacknowledged, and in which the ambivalent reputation of acquisitiveness is rarely, if ever, taken into account. Beyond these considerations there is the fact that economists derive considerable prestige from their role as the high priests of the system's dynamics, hardly a function likely to encourage a skeptical view of its generally accepted institutional structure.

What is then to be done? It must be obvious that I would urge a movement away from the assiduous modelling of an institutionally imprecise, ahistorical society toward the examination of a specifically identified, historically unique social order. Alas, this is precisely the opposite direction of that in which Boettke shows the discipline to have been headed since at least the mid-1930s. After all that I have said with regard to the deep roots of ideology, is it mere wishful thinking to hope for such a turnaround? For all its widespread presence, ideology does not make us hapless creatures. As I have mentioned earlier, we all experience personal changes of heart, and on occasion changes of mind. Admittedly, it is difficult to make such changes in our social visions, but pronouncing them impossible would ignore that more than one deep-rooted ideological position has changed—the unquestioned support of slavery and absolute

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monarchy, as instances—and, looking to contemporary visions, would overlook the slow process of change with respect to racism and male supremacy.

More cogently, perhaps, one might argue that the greatest obstacle to getting economics right is likely to be the breadth and depth of the required knowledge. To give to economics the grasp of social reality it now lacks will require a consideration of many sociopolitical matters of which it is today largely ignorant—worse, indifferent. Such a broadening of the scale of inquiry may well lead to less clearly "modeled" economic depictions than those of which economists boast today. That is why I prefer the vaguer designation of "explanation system" to the more prestigious title of "theory," as the self-description of the discipline-to-be. To the extent that economists turn away from such an undertaking, I can only conclude with pain that the worldly philosophy, which began with such rich promise, seems unlikely to become the source of illumination that was once its singular gift—an illumination that will be urgently needed in the years to come.

NOTES

1. Although I do not have the expertise to pursue my inquiry in terms of the Austrian versus non-Austrian approaches to economic analysis, I should mention that the Austrian work with which I am familiar is expressly excused from this striking omission.
2. See Freud 1957 and Fenichel 1938; for an overview see Heilbroner 1958.
3. Nobel laureate Robert Lucas, asked by economist Arjo Klamer whether governments did not try to resolve social injustice, replied: "That wouldn't be anything like my view. I can't think of explaining the Pharaohs as being in existence to resolve the social injustice in Egypt. I think they perpetrated most of the social injustice in Egypt" (Klamer 1984, 52). The violation of "dispassion" lies, of course, not in Lucas's assertion that the Pharoahs committed monstrosities, but in his choice of Pharaonic Egypt and not Western democracy to represent government. So, too, Nobel laureate Milton Friedman and his wife Rose, admitting freely that of course the distribution of property could be "unfair," go on to assert that unfairness can take many forms—for example, the distribution of talent. "From an ethical point of view," they ask "is there any difference?" (1980, 136). It does not seem to occur to the Friedmans that the unfairness of property can be remedied by social action, but not that of the inequality of talent; or that equalizing resources could help compensate for unequal talents.

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Thomas Mayer

BOETTKE'S AUSTRIAN CRITIQUE OF MAINSTREAM ECONOMICS: AN EMPIRICIST'S RESPONSE

ABSTRACT: *Many of Boettke's criticisms of formalist economics are justified. However, he defines formalism so broadly that it becomes practically synonymous with mainstream economics, while his criticisms primarily target the sins of formalist economics more narrowly defined. And since he treats Austrian economics as the only viable alternative to mainstream economics, he incorrectly awards victory to Austrian economics. While Austrian economics has some valuable ideas to contribute to mainstream economics, it has serious deficiencies of its own.*

Starting in the late 1940s, economics underwent a major revolution, a revolution at least as much in methodology and "scientific taste" as in substantive doctrine. Previously economists, like other scientists and serious scholars, treated rigor as an important virtue. But in the last half-century the value attached to rigor, relative to the other desirable characteristics of a theory—such as empirical confirmation and relevance to practical problems—rose sharply. The leaders of this formalist revolution now seem to classify all economic analyses that are not mathematically formalized modeling as "mere talk." One might be excused for gaining the impression that formalists consider this di-

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Thomas Mayer, Department of Economics, University of California, Davis, telephone (510) 549-0504, telefax (510) 549-9472, wishes to thank Peter Boettke, Kevin Hoover, Michael Montgomery, Nancy Wulwick, and Andrea Salanti for helpful comments.

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chotomy to be more important than the contrast between analyses that are empirically confirmed and those that are empirically disconfirmed.

Despite its insistence on explicit statements and rigor, the formalist revolution was not itself founded on a detailed and rigorous analysis of why formalism is superior to the older way of doing economics. Typically, formalists recite the advantages of rigorous mathematical treatment, but fail to compare them to the advantages of less formal treatment (cf. Backhouse 1997; Montgomery 1997). The great success of formalism should therefore be attributed not entirely, or necessarily even in large part, to superior logic, but, at least in part, to its advocates being brilliant scholars, highly respected for their substantive work. Another explanation is that—at least to those who confuse mathematics and empirical science—formalism fits the image of economics as a science, and that is the image economists want to project. Moreover, because—within the confines of mathematical models—formalist analysis provides a high degree of certitude, many economists find formalism comforting. That it sharply differentiates the professional economist's product from cocktail-party chatter is also satisfying. In addition, it has allowed the young, who generally had better mathematical training than their elders, to achieve dominance.

But although the formalist revolution succeeded in establishing rules to which economists must adhere if they want to publish on mainstream topics in the "respectable" journals, it did not capture the hearts and minds of all economists. It is not certain even that it commands the adherence of half of all the academic economists, and it is likely that outside of academia only a distinct minority of economists accept it. But the tone of economics is set by those who publish in the leading journals and teach in the major research universities. There, formalism dominates.

But by now counterrevolutionaries are active. Peter Boettke is one of them. Being a counterrevolutionary myself, I applaud his attack on formalism, though I define it much more narrowly than he does (see Mayer 1993 and 1995). Counterrevolutionaries, like other revolutionaries, are a quarrelsome lot, so rather than endorse Boettke's criticisms by repeating them, or repeating my own previous criticisms (Mayer 1993), I will criticize some aspects of Boettke's case against what he calls formalism, while omitting the many points about which I can only say "right on."

There are two themes running through Boettke's essay, a powerful

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criticism of contemporary economics for ignoring important older insights that do not fit into the Procrustean bed of formalized analysis, and an acclaim of Austrian economics as the best alternative. I will deal primarily with the second theme. In summary, I criticize Boettke mainly for confounding formalism with a broader version of mainstream economics, thus posing a false dichotomy between formalist and Austrian economics and overstating the contribution of the latter.

Before coming to these criticisms one should note two of the strong points of Boettke's essay. One is his distinction between the idealizations used in economic theory and the criteria that should be used to judge the functioning of an economic system, a point discussed below. Another is his insistence that when discussing broad issues of economic policy, we have to look beyond what economic theory on its own can tell us. Institutions do matter.

Since Boettke gives a prominent role to Abba Lerner as a formalist and proponent of market socialism who got that subject wrong because he ignored institutions, it may be appropriate to digress with a story Lerner once told me. After he wrote *The Economics of Control*, a book that pays scant regard to institutions, he believed that he had demonstrated that it matters little whether a country is capitalist or socialist, as long as its managers follow the correct optimization rules he had set out. But he added that he preferred socialism because (if I remember correctly) it allows a better distribution of income. However, he said that subsequently he changed his mind—because of false teeth. On a visit to Israel he noticed that it did a thriving export business in false teeth. This, said Lerner, was logical because making false teeth is a business that requires much skilled labor and little capital. But, Lerner added, no government planning agency intending to increase exports would ever think of false teeth. Since Boettke stresses the importance of innovation under capitalism, there is therefore much less disagreement between him and Lerner than he suggests.

Boettke's Critique of Formalism

Boettke writes that in describing economic behavior in mathematical language, formalists underplay the complexity of the real world. Hence formalism swept away "historical work on the complex web of institutions that undergird capitalist dynamics" (1997b, 21). For-

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malists, such as Samuelson, have "drained economic theory of institutional context. . . . Parsimony won out over thoroughness. Formalism denied scientific status to *realistic theory. . . . Ideas that defied the techniques of formal analysis came to be considered unworthy of serious consideration*" (ibid., 21, 22, emphasis in original.)

Boettke's delineation of what he calls formalism thus focuses on two separate characteristics. One is formalism's extensive abstraction from institutional context. He is right in believing that this is required for—and fostered by—mathematical modeling. But not only by mathematical modeling. As he himself points out, *all* thinking requires abstraction from a wealth of detail. When Austrian economists talk about entrepreneurship, they, too, are abstracting from the real world, ignoring, for example, such "irrelevant details" as whether the firm (entrepreneur) is a closely held corporation, or one with widely dispersed stockholders who have little power over management.

What matters is whether it is the important or the peripheral characteristics that are being abstracted from. But how do we know which are the important ones? When Austrians complain that formalist economics abstracts from the fact that historical time is irreversible, formalists can reply that time being irreversible is an irrelevant characteristic that does not affect the insights and predictions generated by their theories.

Milton Friedman (1953, ch. 1) has argued that until we know whether a theory's predictions are accurate we cannot say whether the unrealistic nature of its assumptions—that is, its abstractions—matter. An alternative criterion stresses explanation in place of prediction. According to this criterion, the right abstractions yield a theory that makes us *understand* the phenomenon, providing what Fritz Machlup (1978, 145) called a sense of "Ahaness," because it elucidates the mechanism by which the supposed cause generates the claimed effect. Though philosophers of science and methodologists argue about the choice between these criteria, most of us pay some attention to both, though we differ in the relative weight we give them. Even if a theory seems to explain well, in the sense of linking a particular phenomenon smoothly to our prior beliefs, we do not accept it if its predictions are consistently falsified. Conversely, we usually reject as a mere spurious correlation a hypothesis that predicts well, but "makes no sense." But neither approach rejects a theory merely because it abstracts.

One might perhaps respond that some abstractions are so obviously

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wrong that even without knowing how well a theory based on them predicts or explains, we can say with confidence that this theory must be incorrect. But even though there are instances where this is true (e.g., abstracting away self-interested behavior, so that we assume that firms are driven only by altruistic motives), are there many such cases in economics? Boettke is able to say Yes only because he takes it as a given that Austrian theory is correct. Hence, if formalist economics ignores some variable that plays a significant role in Austrian theory, the formalist theory must be wrong. But why assume, a priori, that Austrian theory is correct?

A more nuanced version of Boettke's criticism of formalist abstraction is possible, however. This is the view that often mathematical modeling not only requires more abstraction than verbal analysis, but also that it is harder to be mindful of some of the abstractions that have been made when we read a mathematical analysis than when we read a verbal analysis (see Keynes 1936, 297–98). Moreover, it is tempting to decide what part of reality to model and what part to abstract from by the criterion of what is mathematically tractable, rather than by the criterion of what is important for the question at hand. What makes this temptation worse is that modelers usually do not point out that, since they are abstracting from some characteristics that may be salient, their conclusions are of limited value. To be sure, they may seem to guard against this possibility by showing that their model gives a good fit to the data, but that is a necessary and not a sufficient condition for acceptance. The good fit may well be the result of extensive data mining. Moreover, even though the model gives a good fit, previously published rival models may give a better fit.[1]

What is important here (and incidentally is also basic to Friedman's 1953 essay) is that we use a theory and its abstractions to deal with a particular problem or question. A valid abstraction when addressing one question can be an invalid one when addressing another.[2] When trying to determine whether there is a stable relationship between changes in bank reserves and the money supply, we can safely abstract away the fact that the actions of government officials are influenced by their self-interest. But we cannot abstract away the fact that the ratio of the public's demand for currency relative to deposits varies. Conversely, when we ask, as Boettke does, whether market socialism could work effectively, we can abstract away the stability of the pub-

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lic's demand for currency, but not the motives of government officials.

Boettke's criticism of formalism for its heroic abstractions is therefore too general. He would have to show that these abstractions result in theories that neither predict well nor foster understanding. To be sure, he does discuss why abstracting away the motives that would drive government officials under market socialism invalidates any formalistic case for such a system. And although he does not provide any hard evidence, I find his argument plausible. But market socialism is hardly a central topic in the work of formalists, and it was originally presented without formal models by Oskar Lange (1939) and developed further by Abba Lerner (1944), whose use of mathematics generally consisted of simple geometry. Formalists might therefore readily concede that Boettke's criticism of market socialism is correct, and yet go about most of their business undisturbed.

The second part of Boettke's criticism of formalism—that the medium tends to become the message—is valid. Formalists tend to evaluate ideas by their suitability for modeling, and to judge models much too much by their technical sophistication and elegance, and much too little by the insights they provide into economic behavior or by their predictive success. Deirdre McCloskey (1985) is right in objecting that economics departments have appropriated the criteria that are proper for a mathematics department: rigor, generality, and elegance. Thus, a leading mathematical economist and mathematician, Gerard Debreu (1991, 5), writes:

> In the past two decades, economic theory has been carried away further by a seemingly irresistible current that can be explained only partly by the intellectual successes of its mathematization. Essential to an attempt at a fuller explanation are the values imprinted on an economist by his study of mathematics. When a theorist who has been so typed judges his scholarly work, those values do not play a silent role: they may play a decisive role. The very choice of the questions to which he tries to find answers is influenced by his mathematical background. Thus, the danger is ever present that the part of economics will become secondary, if not marginal, in that judgment.[3]

The issue is therefore not the fact that formalist economists use mathematics, but what they use it for, and the role mathematical techniques play in the criteria by which they evaluate work. Someone might use a mathematically sophisticated and complex model to

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solve a problem that is central to the "study of mankind in the ordinary business of life"—Alfred Marshall's (1947, 1) definition of economics—without abstracting away those institutional details that are relevant for the applicability of the conclusion. He may thereby produce good economics. Someone else who uses no mathematics might tackle a trivial problem, or use too restrictive abstractions, and thus produce bad economics.

There is another kind of formalism on which Boettke does not touch (see Mayer 1993, ch. 3). As an ideal type, this formalism insists on deriving conclusions to every problem explicitly from first principles, which in economics means the assumptions of utility maximization (in practice often narrowed to mean income maximization) and rational behavior, along with a minimum of other assumptions. This type of formalism takes as its model not the natural sciences, but mathematics and logic, with their greater stress on demonstrative reasoning. By contrast, there is what can be called empirical-science economics, again an ideal type. It is more concerned with predicting or explaining empirically observable characteristics of the economy, and less with rigor, parsimony, and elegance.

In practice, formalist economists also want their models to tell us something that is applicable to the real world: they do not, for example, work with models that assume that agents are completely altruistic. On the other side, empirical economists may sometimes start with the same propositions that formalists use. The difference is that formalists are much more likely to treat these propositions as axioms, while empirical economists are more likely to treat them as working assumptions that are usually, but not necessarily always, correct.

Such a distinction is alien to Boettke's framework because, though explicitly contrasting the older neoclassical economics with formalist economics, in much of his essay he divides economists into Austrians, (old) institutionalists, Marxists, and formalists. Since he quickly dismisses institutionalists and Marxists, he has only to show the fallacy of formalism to award victory to the Austrians. But that is too facile. It tars the substantial majority of economists with the same brush, despite major methodological differences among them. Those who take an empirical approach—and there are many—are thereby made to share responsibility for the extreme degree to which abstraction is carried by those who are formalists by my much narrower definition. To accuse the likes of Milton Friedman, Charles Goodhart, Joseph Stiglitz, or Paul Krugman, to name only a few leading mainstream

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economists, of ignoring "the complex web of institutions," or of putting the medium above the message, is surely wrong.

Boettke's discussion of the Chicago school illustrates this tendency to place too many economists into the formalist camp. He strongly approves of the early Chicago school led by Frank Knight, who used the equilibrium concept as an ideal type. He is then critical to some extent of Milton Friedman, and to a much greater extent of Robert Lucas, for treating equilibrium, not as an ideal type, but as a description of the economy. But even Lucas, who is much more formalist than Friedman and Knight, did not always see the economy as in almost permanent equilibrium, at least not in his early work on the aggregate supply function (Lucas 1973). There he dealt with an economy in which it takes time for economic agents to discover when the quantity of money changes and adjust their prices accordingly. Similarly, Friedman attributes fluctuations in output and employment primarily to changes in the quantity of money to which prices have not yet adjusted. In general, it is a mistake to see Friedman as a formalist (my definition) on the basis of his famous methodological essay; far from advocating formalism, Friedman rejects it (Friedman 1953, 11–12, 24–25, 277–300; Hirsch and de Machi 1990; Hammond 1996, ch. 2). It is only real-business-cycle theorists who see the economy in almost continuous equilibrium.

But even if one were to interpret Boettke's attack on what he calls formalist economics only as a criticism of formalism on my narrower definition, it would still go too far. Like most methodologists, Boettke is a monotheist—there is only one true methodology. No tradeoff at the margin is allowed. This might be appropriate if economics had only one purpose. Now in a sense it does: to provide an explanatory and predictive apparatus that ranks high on the criteria of rigor, elegance, generality, and parsimony, as well as on the criteria of accuracy and applicability to many real-world situations. But a theory that fully satisfies all of these criteria would be a rare find indeed. So there is usually a need for tradeoffs, or for multiple theories operating at different levels of abstraction.

One can justify some formalist economics on *l'art pour l'art* grounds because, like any other piece of rigorous and elegant reasoning, it provides intellectual satisfaction. If we support research in pure mathematics and in art history "for the glory of mankind," we should also support some research in formalist economics.[4] Moreover, some formalist models can be justified, not as the end product of economic

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analysis, but as an intermediate product. Allan Gibbard and Hal Varian (1978) have argued that many economic models are "caricatures" intended to highlight some particular feature of the economic process, even though they give a distorted picture of the economy. Other economists can then combine the lessons learned from caricature models to build a more balanced model of the economy. The trouble is that putting these caricature models together into a realistic description of the economy seems less attractive or more difficult than building additional caricature models, so that too little of the former and too much of the latter gets done.

The question is therefore not whether some formalist economics should be done, but how much. The glib answer that some economists give, "Let the academic market decide," will not do. The academic economist's "market" consists of other academics, not consumers who pay with their own money for what they demand, or institutions that are held closely accountable by the general public or by students (the ultimate customers of this research.) Academic economists tend to treat as interesting problems those that are technically difficult, even if they do not have much bearing on how the economy functions or on policy choices. Thus, in academia (or more precisely in those fields of academia where the influence of academics predominates over those of practitioners), the interests of producers tend to outweigh those of consumers, a clear example of market failure. I therefore agree with Boettke that there is far too much formalist research (on my narrow definition), but I would not like to see *all* of it eliminated.

Markets versus Planning

Another problem with Boettke's analysis is that he focuses so much on the problem of free markets versus planning, an issue on which the Austrians have centered much of their attention. This is obviously an important issue for economics. But it is only one of many. Most papers in economics journals either deal with completely different issues, or, if they do deal directly with the question of free markets versus planning, they do so in a specific and narrow context, such as flexible versus fixed exchange rates. Mainstream economics devotes much of its effort to small questions rather than basic systemic questions. For example, the first paper in the September, 1997 issue of the

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American Economic Review deals with an empirical model of international specialization, and the next three papers have the following titles: "The International Transmission of Financial Shocks: The Case of Japan," "A Political-Economic Analysis of Free-Trade Agreements," and "An Empirical Assessment of the Proximity-Concentration Trade-off between Multinational Sales and Trade." Even the two papers in that issue that do compare economic systems ("Privatization in Eastern Germany: Management Selection and Economic Transition" and "Competition or Compensation: Supplier Incentives under the American and Japanese Subcontracting Systems") do so in narrowly circumscribed ways. This narrow focus has proved fruitful. Normal-science research is how a mature field, which economics has become by now, makes its day-to-day advances.

Boettke and David L. Prychitko (1994) tell us that in recent years Austrians have moved away from their traditional emphasis on ideology. But their research agenda is still too strongly influenced by the "big" issue of markets versus planning.

In addition, the issue of planning versus free markets is a problem that belongs only partly within economics. As Boettke rightly points out, it does little good to contrast an idealized picture of market socialism with capitalism as it works in practice. But determining how market socialism would work in practice raises some exceedingly difficult issues for which an economist's expertise is not sufficient. For example, would pressure groups induce the state to prop up firms that should be allowed to fail? This is a question for political science, and not for the unreflective application of economic assumptions to political phenomena. Sociological questions also arise: what elites would replace the capitalist elites, and what effect would that change have? We know by now that social engineering is fraught with uncertainty and danger; the law of unintended consequences rules. There is a saying in the military: no plan survives contact with the enemy. Something similar applies to economic policy.

What economists can do much better than evaluate how market socialism would work in practice is to compare the actual workings of a market system with the workings of an idealized market system. And what one can do well is what one does, particularly if one is an academic. This creates a natural bias against the free market (though one that may perhaps be fully or more than fully offset by biases in the other direction.) Boettke is fully justified in complaining about this bias. Where Boettke is less justified is in rejecting the

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work of those who, like Stiglitz, George Akerlof, or Herbert Simon, among others, have analyzed the implications of factors such as asymmetric information and limited rationality. To be sure, such analyses can be used to demonstrate that the actual functioning of a market system falls short of its ideals. Stiglitz et al. generally do not quantify the losses from these shortfalls, and perhaps they are minor. All the same, they exist. Students of market processes, such as the Austrians, should welcome, rather than deplore, any work that tells us more about how markets operate, even if it creates a bias in favor of planning on the part of those who fail to reflect sufficiently on the shortfalls of actual, as opposed to ideal, socialism. In general, we do not object to increased knowledge because some people may misuse it.

Ideology in Economics

Another problem is Boettke's treatment of rival schools as driven by ideology. In one sense ideology is the metaphysical core of a research program, and therefore unobjectionable.[5] In another sense, used by those who complain about the ideology of their opponents, ideology is a tendency to accept or reject evidence on the basis of whether it fits one's preconceptions, or a tendency to select theories on the basis of their policy implications, instead of the other way round. As such, ideology demonstrates obtuseness or lack of intellectual honesty, at least with oneself, if not with others. To accuse someone or some school of being ideological is therefore to make a serious charge, albeit in *relatively* polite language. Unless one has evidence to back up that type of charge it is better not to make it. To be sure, if an intelligent and well-informed person rejects my excellent arguments, I am tempted to attribute this to willfulness rather than to the unconvincing nature of my arguments—which, after all, I find utterly convincing. But this temptation should be resisted.

However, there is still another way of looking at ideology: to treat it as undesirable in many cases, but also as natural and sometimes as justified. It is natural to give more credence to evidence that supports one's position than to evidence that rebuts it, if only because this eliminates an unpleasant feeling of cognitive dissonance. Moreover, in some cases this tendency may be justified. Suppose I have much evidence that a proposition is correct. I am now presented with evi-

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dence that it is false. Hence, I must usually reject either all the old evidence that supports it, or else the new evidence.[6] Suppose that, upon careful reflection, I cannot find any errors either in the supportive evidence or in the new contradictory evidence. One possibility is to say that I do not know whether the proposition is true. But suppose I have to make some decision, or that the supportive evidence is very strong. It may then be reasonable for me to adhere to my former belief, and to treat the new, contradictory evidence as an anomaly that will sooner or later be somehow resolved, even though this makes me seem ideological.[7] For all of these reasons, it is better not to attribute ideology to one's opponents.

Moreover, it is far from clear that one should blame antimarket ideology on the rise of formalism, as Boettke does. Institutionalism, too, can be used to justify interventionism. Indeed, institutionalism has been the traditional source of interventionist arguments in economics, while neoclassical theorists have usually defended the market system.

New Keynesian Theory

Boettke's treatment of the New Keynesians does not do them justice. New classical economists had attacked Keynesian theory for lacking microfoundations for its basic assumption of wage or price inflexibility. In response, New Keynesians have shown that due to a number of factors (among which the efficiency wages that Boettke stresses are not necessarily the most important), wage and price inflexibility are consistent with rational income maximization, thus refuting the new classical criticism. To be sure, New Keynesians have not succeeded in measuring the absolute or even the relative importance of the various factors they analyze, so their demonstration of the mere existence of these factors does not allow us to claim that we necessarily live in a Keynesian (or monetarist) rather than a new classical world. However, their primary task was not to do that, but merely to demonstrate that models with wage or price inflexibility are consistent with a belief in rational utility maximization, and do not require any implausible ad hoc assumptions. That wages and prices actually are not very flexible is an empirical claim (for which there is substantial empirical evidence) that needs to be added to New Keynesian theoretical models to refute new classical theory.

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Austrians and Post Keynesians

The criticisms of mainstream economics that Boettke and other Austrians make from a right-wing perspective have much in common (both substantively and in style of argument) with the criticisms of the Post Keynesians, who represent the left wing of Keynesianism. Post Keynesians are, like Boettke, critical of formalism and its focus on equilibrium, and like the Austrians, they want to reintroduce historical time into economics. They also stress the prevalence of uncertainty, and its distinction from mere risk where the probability distribution is unknown. Thus, suppose one accepts Boettke's arguments about the inadequacy of mainstream economics in these respects. One does not then have to draw the same conclusions as he does; instead one could become a Post Keynesian. Again, Boettke's tendency to see methodological disagreements in economics as a dichotomy between the Austrians on one side and everyone else on the other muddies the waters.

The Role of Austrian Economics

Boettke's paper is at least as much a pro-Austrian tract as it is a criticism of formalism. It is useful to distinguish two roles that Austrian economics could play. One is to supplement mainstream economics, and the other is to replace it.

As a substitute for mainstream economics, Austrian economics is not likely to be successful, and for good reason. Mainstream economics does have many shortcomings—particularly the version I have called formalist economics. But it also has many successes, especially when accompanied by an empirical mindset, as it is in the work of economists such as Anthony Atkinson, George Akerlof, Friedman, Franco Modigliani, Robert Solow, and James Tobin, to mention just a few modern masters. It has produced testable—and confirmed—hypotheses about important aspects of economic behavior (see Mayer 1995, ch. 11). We would be the poorer if this work were to cease, or if it were to become only a minor tributary to the stream of economics.

Moreover, Austrian economics suffers from several weaknesses. One is the Austrians' tendency to address their discussions primarily to each other, to spend too much time questioning the purity of each

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other's doctrine, and to debate minor deviations among the elect; in other words, to behave like a stereotypical sect.

Another weakness is the Austrians' concentration on "big" problems instead of on the day-to-day problems that constitute the work of a normal science. In Lakatosian terms, too much of Austrian research deals with the metaphysical core, and too little with the protective belt. Austrian economics needs fewer generals making grand plans, and more privates fighting the war one hill at a time. (But Boettke should not be blamed for this failing of Austrian economics. On the contrary, he has been a leader among Austrians in emphasizing "small," manageable problems.)

What is discouraging is that when Austrians have dealt with small problems, often the results have not been fruitful. Boettke's *The Elgar Companion to Austrian Economics* (1994) has a section of short papers on applied economics, presumably intended to illustrate the fruitfulness of Austrian economics when applied to specific problems. I do not think that the section succeeds in this effort. Many of these essays do make valid points, but too often these are points originally made by economists who are more closely identified with mainstream than with Austrian economics. Showing that they are also consistent with Austrian economics is not enough to show that Austrian economics should replace mainstream economics. When these papers do cite specific Austrian contributions, these are usually insights that originated a long time ago—suggesting, again, that Austrian economics is not a progressive research program.

All in all, despite its intellectual ferment in the 1980s (see Lavoie, 1994a), modern Austrian economics has so far, at least, not been very productive with respect to those problems that interest mainstream economists, and also, I would argue, those that interest the general public. Part of this is due, of course, to Austrian theory not providing the rationalizations for government intervention that much of the public and many economists welcome. But that is not the whole story. While in the hands of an intellectual giant like Hayek, Austrian economics is a powerful tool, in the hands of lesser economists it is not as powerful a tool as mainstream theory is. And that is a serious drawback. Inevitably, most researchers are not giants, and a productive methodology is one that provides ways in which they, too, can make useful contributions. We should evaluate methodologies not only in the context of verification, but also in the context of discovery.

The lesser fruitfulness of Austrian economics in the hands of most

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economists is due not only to its previously discussed focus on "big" problems, but also to the much smaller role that Austrians accord to that great reservoir of tasks for many economists, empirical work. Many Austrians consider econometrics to be useless. Leading Austrians reject on methodological grounds the validity of aggregates, such as the price level. Thus Hayek (1935, 5) wrote: "From the very nature of economic theory, averages can never form a link in its reasoning." Austrians therefore reject as inapplicable to the study of society the instrumentalism that is used so much in the natural sciences. This position strikes me as mistaken, but not being a philosopher, I will not attempt to discuss it further.

If they reject econometrics, Austrians could instead use qualitative economic history as a testing ground, particularly if they are willing to use averages. Although some have done so—for example, by arguing that an unregulated banking system has worked well in the past (for a summary see Schuler 1994)—on the whole, Austrians do little historical work, because of their strong abhorrence of historicism with its disdain of economic theory. But economic history can be used both to support and to make use of economic theory, instead of as a substitute for it. So it is not surprising that *some* Austrians—including Boettke—are now urging their colleagues to make more use of economic history, and to test both the correctness and the explanatory power of their theories (see Boettke, Horwitz, and Prychitko 1994). Indeed, Boettke (1994, 5) has argued that the claim that Austrians reject empirical research is "a misreading of the Austrian tradition," though he adds that Austrians also believe that "the significant debates in the social sciences are theoretical debates."

All the same, Austrians do less empirical work than mainstream economists do, perhaps because they want to engage in the more significant debates. Another reason might be that many Austrians adhere to a mentalist subjectivism that suggests that empirical work is not likely to be successful (see Lavoie 1994b). By rejecting econometrics and making only very limited use of economic history, Austrians have denied themselves the opportunity to test their theories, though this, too, has changed somewhat in recent years (see Lavoie 1994a).

Austrians might respond, along with some mainstream economists (for instance, Robbins 1932), that empirical testing is not needed, that it suffices to construct a valid logical chain beginning with axioms we know from introspection and casual observation to be true, and ending with whatever conclusion follows deductively. But while

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that may allow us to argue that a certain effect does occur, it usually cannot tell us whether it is significant or not. For example, Hayek (1935) attributed business cycles to the changes in the relative prices of various goods induced by changes in the money supply. Skeptics might respond that, while such relative price changes contribute to business cycles, it is possible that they explain only a trivial proportion of the fluctuations we experience as business cycles. Similarly, for a long time Austrians have talked about the importance of complementarity among various types of capital. But how much complementarity is there? Only recently has an economist (and one who has only tenuous links with the Austrians) provided empirical evidence that such complementarity does matter empirically (Montgomery 1996).

Austrian Economics as a Complement to Mainstream Economics

Viewed as a supplement to, and not a replacement for, mainstream economics, Austrian economics has something to contribute. Mainstream economics has narrowed its vision to a limited set of ideas that modelers find tractable. The Austrians therefore have something valuable to contribute when they insist that the entrepreneur is more than some nondescript graduate of an MBA program who mechanically grinds out the profit-maximizing solution to a standardized problem. Similarly, their insistence that price competition is only one aspect—and not necessarily the most important aspect—of the competitive process adds a valuable insight that game-theoretic models of oligopoly are likely to miss. Insistence on entrepreneurs being innovators operating in a fog of uncertainty, as well as insistence on irreversible historical time, are other Austrian ideas that may deserve much more attention than the mainstream gives them.

One might liken the difference between mainstream and Austrian economics to the difference between a flashlight and a lantern. The former illuminates sharply along a narrow beam. The latter provides all-round but less sharp illumination. Which one is preferable depends on one's purpose, and neither should necessarily be rejected because the other is better at solving *some* problems. Consider, for example, Boettke's criticism of the equilibrium concept used in mainstream economics. He is right in saying that its beam misses much of what we should see, such as the entrepreneur's creation of new goods

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and new markets, so that some of the standard criticisms of product differentiation are simplistic. On the other hand, the insight that markets tend toward equilibrium provides us with a powerful tool for predicting how they will respond to certain shocks. If we were to relinquish standard equilibrium analysis we would lose much. But if we fail to see that there is much more to market behavior than a tendency toward narrowly defined equilibrium, we also lose much.[8] Mainstream economists and Austrians have something to learn from each other.

Improving Communication

Unfortunately, the lines of communication between Austrian and mainstream economics are feeble. I doubt that many mainstream economists read any Austrian economics, though this situation seems to have improved recently. Some economists who are not identified as Austrians now take Austrian ideas seriously (see Caldwell 1982, Hoover 1988, Montgomery 1996). But communication still needs much improvement. One factor hindering it is the somewhat arrogant belief of many mainstream economists that the writings of heterodox economists, or for that matter just about all economics written prior to the 1970s, has nothing to teach them.

On the other side, it is still the case, though perhaps to a lesser extent now than before, that an Austrian discussion of a mainstream proposition often consists in large part of showing that it is inconsistent with Austrian economics, perhaps even that it has been explicitly rejected by one of the Founding Fathers. Given the disdain with which most mainstream economists treat Austrian economics, such introversion is understandable—nor is it necessarily worse than the introversion of the formalists, who tend to treat economics as the study, not of the economy, but of other economists' models. But introversion on either side hinders progress.

Even when Austrians go beyond condemning some work as being un-Austrian, their criticisms usually deal in generalities, such as the mainstream's use of illegitimate aggregate concepts, instead of getting down into the trenches. It is easy for mainstream economists to ignore such broad criticism and say: "Yes, right, but our theories nonetheless *work*." It would be more difficult to ignore the Austrians' criticisms if these would more often consist of evidence (or, more

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specifically, what mainstream economists consider evidence) that a particular mainstream theory or model fails to predict or explain because it ignores a specific point made by Austrians.

While few mainstream economists are likely to become converts to Austrian economics in its entirety, more might take specific Austrian ideas seriously if these were shown to be relevant to their specific concerns, and were reformulated in a way that is compatible with mainstream methodological criteria. Granted that Austrian economics is built on philosophical foundations that differ sharply from those of mainstream economics, it would "sell" better if the methodology and the contents were marketed separately. Similarly, a more politically neutral version of Austrian economics might find additional markets.

Austrians may, of course, object that they want to do more than bring about a few changes in mainstream economics. But aiming at an attainable goal is better than aiming at one that is out of reach. Moreover, a series of small changes may add up to a large change. One should not exaggerate the incompatibility of paradigms.

This is not to deny that over the years Austrian economics has already made some major contributions to mainstream economics. Its insight into the problem of rational resource allocation under socialism is one, and so is Hayek's work on the role of decentralized information in general. But more interaction is needed. Since Austrians are in the minority, as a practical matter it is up to them to build the needed bridges.

* * *

In summary, then, Boettke's criticism of what he calls "formalism" goes too far. He defines formalism too broadly because he sees methodological disputes as pitting Austrians plus a few neoclassical economists against everybody else, thus ignoring some basic distinctions among the "everybody else." In particular, he overlooks the important empiricist tradition in modern economics.

All the same, though I have not discussed these points, there is much in Boetke's criticisms of contemporary mainstream economics that is correct and well argued. Moreover, his essay does serve an important and useful purpose in drawing attention to Austrian economics, which has some important ideas to contribute. But substituting Austrian economics for what Boettke calls formalism, or awarding it the main role in such a synthesis, would be a mistake

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NOTES

1. For other problems with modeling see Mayer 1996.
2. Physicists on a practical level also use different theories when dealing with large objects and with subatomic particles, though, of course, their theories fit together. In economics, too, we can work with general enough premises to cover the entire field, but then our analysis tends to be vacuous. Whether this reflects a fundamental difference between the natural and the social sciences, or is merely a difference in the stages of their development, is another question.
3. The last sentence should be read only as a qualification and warning against overvaluing the beneficial role of mathematics in economics, since elsewhere Debreu (1984 and 1986) has stressed the virtues of mathematical economics.
4. In Bergamo, Italy, I saw just below the dome of a church a fresco that can be seen—and seen only faintly—from the top of a certain tower. Presumably it was painted "for the glory of God," not for people's enjoyment or instruction. One can justify some papers in *Econometrica*, which almost nobody can read, in the same way, given how few readers there are even for most of the less-technical papers in economic journals, and how unlikely it is that they will have any influence on policy or on economists' thinking.
5. One might, however, object that in the social sciences, knowledge is so precarious that one should limit as much as one reasonably can those propositions that are privileged as uncontroversial. Many ideological propositions would then not qualify for the core.
6. But that is not always so. In some situations it is reasonable to hold two conflicting views (see Foley 1979).
7. Here is a concrete example. Phillip Cagan (1965) found that being covered by a corporate pension scheme induces households to save more on their own. This implies an increasing marginal utility of wealth, and therefore conflicts with the well-established belief that marginal utility decreases as one obtains more. I therefore did not accept Cagan's findings, though I could find no fault with his analysis. Subsequently, when Cagan's data were reanalyzed, it turned out that his conclusion had been wrong.
8. Boettke (1997b, 30) recognizes this when he writes that "equilibrium theorizing is not to be rejected, according to Hayek, but its real purpose must be constantly kept in mind. Formal modeling can be a very good servant, but a poor master." However, the general impression that Boettke gives is of a strong condemnation of equilibrium theory.

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Peter J. Boettke

FORMALISM AND CONTEMPORARY ECONOMICS: A REPLY TO HAUSMAN, HEILBRONER, AND MAYER

ABSTRACT: *Economic formalism crowds out the analysis of change and adjustments to change under capitalism. The style of analytical narrative that was practiced by the first generation of neoclassical economists, in contrast, is more productive of genuine economic understanding. Despite Daniel Hausman's challenging argument to the contrary, I maintain that Joseph Stiglitz's work is formalist at its core. While I agree with Robert Heilbroner's critique of contemporary economics, there is a limited sense in which nonformalist economics can rely on universalistic assumptions. And Thomas Mayer has provided useful guidelines for focusing nonformalist analysis on real-world economic problems.*

My purpose in writing "Where Did Economics Go Wrong?" was rather straightforward. In the wake of the rejection of the real-existing socialist political economies of East and Central Europe in 1989 and the subsequent collapse of the Soviet government in 1991, F. A. Hayek was often paid lip-service for his prescient skepticism about central planning. Unfortunately, in my opinion, Hayek's analytical contribution was generally misunderstood. In an article written before Hayek's death in March 1992, I had already taken up the task of

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Peter J. Boettke, Department of Economics, George Mason University, Fairfax, VA 22030, is the author of *The Political Economy of Soviet Socialism* (Kluwer, 1990) and *Why Perestroika Failed* (Routledge, 1993). The comments and criticisms of Andrew Farrant, Roger Garrison, Mario Rizzo, Lawrence White, and the editor are gratefully acknowledged. The usual caveat applies.

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attempting to demonstrate that Hayek and his teacher Mises were not simply broken clocks that fortuitously could tell the time correctly twice a day (see Boettke 1992). There are analytical features of Hayek's political economy that provide the foundation for his deep commitment to classical liberalism, and thus his opposition to various forms of socialism and Keynesian demand management.

Explaining why Hayek's argument was misunderstood—let alone not appreciated—was what motivated me to write "Where Did Economics Go Wrong?" The argumentative strategy was to use Hayek as a fulcrum for comparing the economics of the 1930s and 1940s to that of the 1980s and 1990s. The idea was to contrast Hayek first with old Keynesianism and old market socialism, and then again with new Keynesianism and new market socialism, and to demonstrate that the essential analytical points that Hayek raised in the older debates are being overlooked in the contemporary debates, despite the grudging respect paid to Hayek as an ideological warrior for the competitive market economy. Since Hayek failed to persuade his fellow economists in the 1930s and 1940s, the development of economic theory in the 1950s and 1960s proceeded along lines that ignored his contributions. With the failure of the Keynesian system in the 1970s and real existing socialism in the 1980s, many of Hayek's criticisms were reconsidered and supposedly incorporated. However, a close reading demonstrates that Hayek's main analytical contributions have failed to penetrate the core of contemporary economic theory. In other words, the implications of Hayekian economics are more radical for the way we conceive of political economy than has been appreciated, and if absorbed would demand a recasting of this enterprise.

The nature of my self-imposed task was, therefore, necessarily broad and not the focused critique to which one is accustomed in economic writings. Nevertheless, I stand by my basic contention that modern economics developed in a manner that ignored many essential characteristics of real-world economic and political life because of the systemic biases of formalism, and that this has had a detrimental effect on the progress of economic thought. It is not my contention, however, that either Hayek or Austrian economics has an exclusive claim to sound economic reasoning. I am persuaded that the Austrian economists have identified certain fundamental issues that need to be incorporated into the body of economic thought if we are to improve our understanding of the human condition. In the end, however, like Milton Friedman, I believe there is only good economics

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and bad economics. Good economics means paying attention to human choice in complex problem situations, recognizing the institutional sensitivity of economic propositions, and respecting the multiple forms of evidence that bear on those propositions. Much of the most important data in economics is of a qualitative nature and thus is not amenable to modern techniques of statistical analysis. Rather than either attempting to quantify data that cannot be easily quantified or ignoring them, we should embrace narrative history—and the discipline of the archive—as legitimate parts of empirical economics. In fact, the "revolution" I advocate for economics amounts to little more than returning aspiring economists first to library shelves, to read the classics in the "worldly philosophy" that can teach them to situate modern contributions within a broader disciplinary perspective, and then to library basements to uncover memoirs, letters, policy debates, contracts and other business documents, and political history that can teach them about the evolution and development of the institutions that shape economic life. Hence the critique of formalism in my paper—for it is the formalist bias within contemporary economics (and the social sciences in general) that has led to a disregard of the unquantifiable elements in economic life.

Hausman and the Misplaced Centrality of Perfect Competition

Daniel Hausman is undoubtedly one of the most profound contemporary philosophers of economic science. It is with great hesitation that I attempt to defend myself against his rather harsh criticisms of aspects of my argument. The most troublesome of his criticisms is the claim that my reading of Stiglitz is mistaken, and that my article is mainly a plea for economists to become uncompromising defenders of laissez faire. I will attempt to defend my reading of Stiglitz, though I do plead guilty to being persuaded that certain basic teachings of economics support the superiority of the market mechanism over alternative systems of exchange and production. But I want it to be clear that I do not believe that economics alone can produce a case for laissez faire. The discipline of economics provides us mainly with *negative* knowledge, knowledge that puts parameters on our utopias. Through the process of immanent criticism, the economic analyst can establish the consequences of a proposed policy; and in the wake of repeated discoveries of unintended negative consequences, economics

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does lead us to doubt that, some day, somebody will hit on a way to outperform the market. Both the critique of unintended negative consequences and the critique of intervention in markets as a tool of special interests follow from a consistent application of economic principles. But neither produces an air-tight case for unfettered capitalism. To make that case requires something more than pure economics. One reason for this is that markets are always embedded within a broader political-legal infrastructure, so the idea of a complete absence of government is conceptually questionable. Some form of governance structure must be in operation, and it is not at all self-evident that the most effective governance structure will always emerge endogenously from the market process itself. Second, there are competing values within any political economy that may need to be traded off against each other. Perhaps we can summarize these values under the broad headings of efficiency, equity, and fair play (see Boettke 1998). Economics as a science cannot serve as the final arbiter in this negotiation process; as a discipline it provides vital knowledge that must be incorporated in assessing the tradeoffs, but it cannot tell us which values should prevail. As Hausman himself has argued elsewhere, economists should pay attention to developments in moral philosophy, just as moral philosophers have to pay attention to developments in economics. "Economic outcomes may be better or worse along several dimensions," write Hausman and Michael McPherson (1996, 69). "Some outcomes may make people better off. Others may show more respect for human dignity. Others may permit greater freedom. To decide which dimensions are more important requires moral judgement."

So I do not think that economics produces a case for uncompromising laissez faire. My criticism of contemporary economics is that positivism and formalism dismiss out of hand certain arguments and evidence bearing on the desirability of laissez faire because of the form they take rather than their substantive content.

With regard to the centrality of the perfect competition model in contemporary economics and in the work of Stiglitz in particular, however, I am not yet persuaded that rhetorical excess on my part misled my readers. The fact that my critique focused on Stiglitz had to do with my judgment that Stiglitz is the most significant mainstream economic theorist since Paul Samuelson. No doubt Stiglitz is a critic of the model of competitive equilibrium, and in fact his major contributions have brought out some of the problems with this

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model. But he cannot abandon this model, because he can make reference to market failures only by point of comparison with perfect markets. If the model of competitive equilibrium is completely jettisoned, then the point of reference for judgments of "failure" would be jettisoned as well. Perhaps some alternative standard would emerge, but Stiglitz has not provided it. In Stiglitz's own elementary treatment of this issue in *Economics* (1993, 386–89), he divides the economics profession into two camps: free-market economists who believe that the model of general competitive equilibrium provides a good description of most markets most of the time, and imperfect-market economists (such as himself) who see significant departures from the competitive model in the real world they observe.

For Stiglitz, among the key imperfections of markets is the costliness of information, but he also highlights problems with the structure of markets (e.g., small rather than large numbers of buyers or sellers). The Greenwald-Stiglitz theorem aims to demonstrate that under conditions of imperfect information, limited markets, and other market imperfections, (1) well-identified forms of government intervention can indeed be welfare enhancing; (2) efficient decentralization may be impossible; and (3) the market mechanism might not be the most efficient way of allocating scarce resources (Stiglitz 1994, 32–33). But unless Stiglitz has some benchmark notion of perfection, the terminology of "imperfect" markets would be meaningless. Stiglitz admits this, writing that "imperfect-market economists would not, however, discard the model: for them, it is still an important baseline for investigation" (1993, 388).

Now here things get a bit complicated. Stiglitz wants to argue against Hayek's depiction of the informational advantages of market decentralization, yet he knows that Hayek is not guilty of deploying the model of perfect competition to make his case for the market. In fact, in the socialist calculation debate, Hayek explicitly blamed the model of competitive equilibrium for clouding the judgment of leading economists, such as Joseph Schumpeter. Thus, Stiglitz devotes an important section of his *Whither Socialism?* to dealing with the question of "Hayek versus Stiglitz." But the debate is never engaged on the appropriate grounds because Stiglitz insists, first, that "because Hayek (and his followers) failed to develop formal models of the market process, it is not possible to assess claims concerning the efficiency of that process," and because, "second (and relatedly), in the absence of such modeling, it is not possible to address the central is-

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sues of concern here, the *mix* and *design* of public and private activities, including alternative forms of regulations (alternative 'rules of the game' that the government might establish) and the advantages of alternative policies toward decentralization-centralization" (Stiglitz 1994, 24–25).[1]

It is statements like this one that led me to write my paper in the first place. Hayek offered an alternative framework for analyzing market processes, one that transcends the perfect/imperfect-market dichotomy. Hayek's argument about why decentralized markets work is not limited to the informational content of *equilibrium* prices, but instead refers to the error-detection capacity afforded by non-equilibrium prices and thus the adaptive properties of the price *system*. It is precisely market *disequilibrium* that initiates the process of knowledge discovery that characterizes real-world competition. What imperfect-market theorists such as Stiglitz take as evidence of market imperfection (and thus failure) is to Hayek the very essence of the competitive market process. I tried to spell out those properties in my paper so that perhaps we could go back and reassess Stiglitz's dismissal of Hayek (and his followers). Such a reassessment is not an "act of faith," but the outcome of a chain of reasoning that must be analyzed on its own terms. Just because an argument about disequilibrium adjustment—as opposed to a suboptimal equilibrium—defies neat formalization should not be grounds for ignoring it or dismissing it. Reason and evidence need not be presented only in one "language," the language of mathematics, to be accorded respect, especially if in hindsight we are willing to grant prescience to Hayek in predicting the collapse of socialism. Mathematical modeling need not be the only language for expressing economic propositions, and tests of statistical significance need not be the only manner in which to evaluate evidence.

Heilbroner and the Critique of Economics

I share so much with Heilbroner's perspective that, on the failings of contemporary economics, I will only recommend that the interested reader examine Heilbroner and Milberg's *The Crisis of Vision in Modern Economics* (1994).[2] My sole objection to Heilbroner's comment on my paper relates to the limited but vital role of analytical propositions that are not restricted to the institutional configuration we call capi-

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talism. These propositions are derived from conditions common to all human situations—the logic of scarcity and what that implies for human choice making. The concept of opportunity cost was as relevant to a Soviet planner as it is for a Western businessman deciding on whether to pursue investment project A or B. Soviet planning authorities might have seemed to have controlled all decision-making processes, but the *de facto* reality of the system stood in direct contrast to this *de jure* illusion. Analysts of the Soviet system were often misled in their examination of the Soviet economy precisely because they failed to take into account the *de facto*, scarcity-driven organizing principles and why these diverged from the official picture embodied in plans.[3]

What is true for the Soviet system is also true for the Kalahari bushman and the Sumerian, Akadian, Egyptian, Incan, and other systems of production and distribution. There are certain fundamental propositions in economics that are universally valid because the initial conditions required for their applicability are so general that they hold across times and cultures. These propositions are very limited, and for the most part fairly uninteresting in themselves, but they provide the building blocks for very interesting claims that are of a more institutionally contingent nature. Economics has a lot to say about all social systems of production—not the same thing, granted, but something—even when the dominant influence on decision making is seemingly but the "will of one." It is certainly true, as Heilbroner argues, that Tradition, Market, and Command have served as divergent organizing principles in various societies throughout history. But that does not mean that an economic analysis of traditional societies or command societies is void of meaningful content because the market economy did not play the main role in coordinating human affairs. Basic economics is not limited to market interaction, but examines human choice whenever scarcity is a consideration.

I don't want to make too much of this difference with Heilbroner. The disagreement ultimately traces back to my claim that Austrian economists are firmly rooted in the marginalist/neoclassical project, though they situate the individual within the problem situation articulated by heterodox economists (see Boettke 1996b). The manner in which the marginalist/neoclassical project has proceeded has been one that trivialized the problem situation, so that equilibrium followed inexorably from the logic of choice. On the other hand, the problem with the heterodox project is that once the problem situa-

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tion is laid out—namely that time's arrow runs only in one direction, and that individuals possess imperfect knowledge of existing conditions and imperfect foresight into future conditions—it is simply assumed that the individual logic of choice cannot result in the systematic coordination of individual economic activities. I do believe that Hayek's economics, or Austrian economics more broadly understood, offers us a way to combine the argumentative structure of economic science with the sensitivity to initial conditions and historical setting that animate the heterodox critique. This is what Hans Mayer meant when he argued that the path to scientific enlightenment in economics is along "the road on which the great system-builders of the 'older' German historical school meet up with the founders of the 'Austrian School'" (Mayer 1932, 149). This, I believe, was a path Max Weber tried to carve out.

Along this road, sociopolitical matters that, as Heilbroner points out, are today ignored will once again come to the forefront of analysis. As Heilbroner predicts, this will most likely lead to less clearly modeled depictions of the economic system. But the promise of richer explanations is worth the loss of crystalline precision, since the precision that has been achieved falsifies reality.[4] If we continue to insist that precision is always more valuable than relevance, then I conclude with Heilbroner that "the worldly philosophy, which began with such rich promise, seems unlikely to become the source of illumination that was once its singular gift."

Big Think, Little Think, and Mayer's Pragmatic Criticisms

Mayer's criticisms of my paper are those that have been the most challenging for me to think about. The reason is that I find myself in almost complete agreement with Mayer's observation that good economic analysis is practiced every day by non-Austrians, and that many Austrians engage in rather poor economic analysis. The burden of proof must be shouldered by the critic, not those following the conventional wisdom. Perhaps the worst form of work in economics is bad big think; at least bad little think is usually short in length, whereas bad big think tends to go on and on.

But the discipline of economics has lost track of problems and it has forgotten questions that have to be resurrected if progress is going to be made.[5] Certainly mid-century economics sought to develop an

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institutionally antiseptic theory, despite Abba Lerner's sudden appreciation for market institutions when confronted with the unpredictable Israeli specialization in false teeth. Sometimes our "appreciative theory" outdistances available "formal theory," and in some instances that will always be the case.[6] The problem with formalism is that its tenets compel scholars to treat appreciative theory *always* as the immature sibling of the more developed formal theory.

Mayer agrees with me that mainstream economics has failed to solve some of its key problems. But he turns the criticism back on the critic. How much progress have the Austrians made in solving problems that interest practicing economists? I have to admit that in terms of real progress in scientific understanding, apart from Israel Kirzner's contributions to the theory of the entrepreneurial market process and recent work on "free banking," the golden years for the Austrians were in the last three decades of the nineteenth century, and then again in the 1930s and 1940s. The Austrian tradition lay practically dormant from the 1940s to the 1970s, except for occasional contributions from Mises, Hayek, Ludwig Lachmann, Murray Rothbard, and Kirzner. Since the mid-1970s there has been a growing resurgence of the Austrian school, and there has been within the school a strong current of self-criticism aimed at killing off the dogmatism that may have served a positive sociological function during the years when the tradition was largely dormant. But Austrians as a school still devote considerable time to methodology, history of thought, and "big think" ideological questions, and this does not conform with the interests of most practicing economists.[7] While I admit that this is a problem within the Austrian camp, I am also willing to "defend" it, or at least understand it. When a set of ideas is ignored in the standard curriculum of undergraduate and graduate training in economics, only extraordinary circumstances will lead young economists to devote time and effort, beyond their standard training, to the study of this heterodoxy.

Those extraordinary circumstances—shared by most heterodox schools—usually combine a set of methodological criticisms of standard practice, a historical narrative about how important insights have come to be overlooked, and a belief that what is at stake is something of major importance to human well-being. Those who reject the standard practice in "the discipline" of economics must be led to that rejection because they find the discipline to possess great potential for solving human problems and they want to know why that potential is

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unfulfilled in current practice. If they just viewed the discipline as a problem, then they could pursue alternative professional training. To pursue professional training in a discipline that one believes is fundamentally flawed requires some deep commitment outside of the internal reward mechanisms of the profession. This may cause problems in communicating with other economists, but without it there would be nothing to communicate.[8]

That much said, it is true that despite the sociological function this emphasis on methodology, history of thought, and "big think" serves, scientific progress must be measured by solving an increasing number of "little-think" problems. By focusing on smaller, more manageable problems, economists can demonstrate the effective comparative power of their theoretical insights. What Austrian economists need now is more historical and applied work and fewer methodological exposés and theoretical treatises. This is not to say that methodological and theoretical developments are not needed; they certainly are, because the basic approach of economists will sometimes have to be changed if we are to tackle the lost problems and the forgotten questions. But for the majority of practicing economists attracted to the Austrian school, the greatest opportunity for marginal gains in understanding the real world will lie in the application of existing Austrian propositions. Solid empirical work needs to be done. In my own field of comparative political economy and economic development, there are indeed important theoretical disputes, but there are also fundamental disputes of an empirical nature—e.g., the question of monopolies in the Gilded Age and the present; the causes of the Great Depression; the nature of Soviet industrialization; the effectiveness of the Marshall Plan for postwar reconstruction; the sources of the Asian miracle and of its weaknesses; the cause of the collapse of real existing socialism in the late 1980s; and the sources of Third World poverty. If Austrian economists can explain these phenomena better than alternative schools of thought, then the theorizing and methodologizing will have been well worth it.

However, contemporary economists often put unnecessary constraints on the form of legitimate argument and the type of evidence that can be marshaled in its support. Many Austrian insights must be presented in the form of natural language as opposed to a set of equations, and they demand evidence other than measures of statistical significance. One of the most promising developments in contemporary economics is the increasing acceptance of economic history, in-

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cluding history that comes closer to the standards of traditional archival history (see Greif 1997, 400–403). But since the whole *raison d'être* of Austrian economics is to produce a more realistic picture of the political-economic world, if this proves impossible within the discipline of economics — indeed, even if it does not — it is a project that should be pursued by political scientists, sociologists, and historians proper, as Weber anticipated.

* * *

The problem with contemporary economics, then, is that it artificially restricts the questions we can legitimately ask about the real world. The "older economists" were not so constrained. My article sought to place the blame for these relatively recent constraints on a formalistic bias that took hold of economics in the 1940s. I stand by that basic historical narrative, which suggests that the likes of Frank Knight, Ludwig von Mises, Ronald Coase, F. A. Hayek, James Buchanan, and Mancur Olson (despite whatever ideological blinders may have handicapped some of them) have done more to increase our understanding of political-economic life than the work of such brilliant formalists as Jan Tinbergen, Kenneth Arrow, Robert Lucas, Paul Samuelson, and Joseph Stiglitz. This judgment is not shared by many economists. Why?

The question still puzzles me. The discipline of economics was born of the attempt to explain how various institutions affect the wealth and poverty of nations. This question still remains with us (see Landes 1998). Adam Smith's heirs, in my opinion, are to be found in the first group of economists just listed, not in the second. Perhaps I have blind spots induced by overzealous advocacy of the Austrian corrective to contemporary economics. But in the end, all I am really advocating is "good" political economy that results in "better" narrative history. The purpose of theory is to aid the task of writing history (including the diagnosis of contemporary problems). Traditionally, the worldly philosophy attempted to illuminate the human condition. Developments within twentieth-century economics moved us away from the task of understanding and illumination in a vain quest for scientific precision. If we fail to resist those developments so as to re-engage the worldly philosophy, then the humanistic desire to understand ourselves, the scientific impulse to know the underlying forces at work, and the political belief that understanding and knowledge can be employed to improve the human condition

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will be eliminated from political economy. A sadder fate for a discipline that has included some of the greatest minds in the Western tradition could hardly be imagined.

NOTES

1. In my review of Stiglitz's *Whither Socialism?*, I also point out that his disregard of public-choice arguments and of Coase's comparative institutional analysis undermine his ability to achieve a realistic examination of the mix of public and private. See Boettke 1996a.
2. See also the symposium on Heilbroner and Milberg 1994 in Boettke and Horwitz 1997.
3. This has been something I have repeatedly stressed in my own work on the Soviet system. See Boettke 1990 and 1993.
4. This is not a generalized critique of abstraction, but an attempt to question the right amount of abstraction. Simplifying the problem situation for the sake of formal tractability is an abuse of the principle of parsimony. As Oskar Morgenstern (1964, 255) put it: "The abstraction would be faulty if it bypasses a fundamental feature of economic reality and if the analysis of the radically simplified situation will never point towards its own modification in such a manner that eventually the true problem can be tackled. . . . Radical simplifications are allowable in science so long as they do not go against the essence of the given problem."
5. See, for example, the survey articles from the 1970s by Oskar Morgenstern (1972) and Joan Robinson (1977), which identify the main unsolved theoretical points and unanswered questions in economics and assess the progress made in addressing them. If, indeed, Morgenstern and Robinson identified important problems and questions, and little progress has been made, then economists who work with those problems or ask those questions do indeed have a market niche within the economics profession.
6. The terminology of "appreciative" and "formal" theory was introduced in economics by Nelson and Winter 1982, 45–48.
7. In comments on an earlier version of this paper, Mario Rizzo pointed out to me that this communication problem can also be viewed as a natural outcome of the present status of the discipline. If there is an evolutionary selection (or self-selection) bias in the discipline, then we must ask who will stay in a discipline when things are degenerating. Only those who accept the degeneration as appropriate—and a few revolutionaries—will remain. Nevertheless, if the latter hope to engage the former, then the communcation gap must be closed—by the latter.
8. The argument of the last paragraph is true not only for Austrian economists, but also Post Keynesians, Institutionalists, Marxists, etc. Methodology, history of thought, and idealism are present in all deviant strands within economics. New Institutional economists, for example, tell a different history of the de-

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velopment of economics than more traditional economists. Some dissenters play up the methodological differences, others play up the historical story, and others, still, play up the political consequences.

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Maurizio Viroli

ON CIVIC REPUBLICANISM: REPLY TO XENOS AND YACK

ABSTRACT: *Current debates about patriotism and nationalism have so far failed adequately to take into account the historical meaning of republican patriotism. For classical and modern republican theorists, love of country is a charitable love of the republic and of its citizens. It is an attachment to the political values of republican liberty and to the culture based upon them. As such, it is a theoretical alternative to both civic and ethnic nationalism, and it is not at all confined within the bygone experience of city-states.*

I have gladly accepted the opportunity to discuss Nicholas Xenos's and Bernard Yack's essays on civic nationalism published in the Spring 1996 issue of *Critical Review*, because both raise important questions that can help us to refine our understanding of patriotism and nationalism. I also wish to stress that I am particularly happy to have the opportunity to respond to Xenos's critique of my book *For Love of Country*, not only because his remarks have compelled me to think about a number of historical and theoretical issues which I had not considered at all, but also because I find his essay a laudable example of civilized scholarly discussion, a style that is becoming more and more rare as it is too often replaced by polemical language marked by arrogance and condescension.

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Maurizio Viroli, Department of Politics, Princeton University, Princeton, N.J. 08540, is the author, *inter alia*, of *L'Etica laica di Erminio Juvalta* (Angeli, 1988), *Jean-Jacques Rousseau and the "Well-Ordered Society"* (Cambridge, 1988), *From Politics to Reason of State* (Cambridge, 1992), and *For Love of Country: An Essay on Patriotism and Nationalism* (Oxford, 1995).

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I must, however, confess that I had some hesitations about entering into a debate with Xenos and Yack: their essays criticize civic nationalism, and I am not, nor have I ever been, a proponent of such a theory. On the contrary, I entirely concur with Yack's opinion that civic nationalism is a "mixture of self-congratulation and wishful thinking" inspired by liberal thinkers' understandable desire to define national identity in a way that makes it compatible with rational and universalistic political principles of liberty and equality.

I am also inclined to accept the thrust of Xenos's argument, even if I would rather call civic nationalism a bad choice of words signalling an unappealing theoretical mixture, rather than an "oxymoron." What I mean to say is that civic nationalism is a combination of two concepts (civism and nationalism) that obscures reality and lacks intellectual elegance. Combinations of different concepts and traditions can be powerful and attractive, if—as, for instance, in the case of Carlo Rosselli's theory of liberal socialism—the two concepts enrich one another. In the case of civic nationalism, however, the combination is one in which the two terms subtract from and impoverish each other. The second term (nationalism) pollutes the first (civism) by adding to it elements that are hardly congenial; while civism, once attached to nationalism, does not redeem it at all. It is, if I am allowed a nationalist remark, like spaghetti with meat balls; they are better separated, or to paraphrase Secondo's line in the movie "The Great Night," they want to be alone.

Having said that, however, I still think that a dialogue with Yack and Xenos can be useful, because they frame their arguments within a set of concepts that are not exhaustive of the historical traditions and theoretical possibilities available to us. Yack discusses the distinction between civic nationalism, understood as a freely chosen rational allegiance to political principles, and ethnic nationalism, which celebrates the inherited cultural identity of a nation; he concludes that civic nationalism is a myth, as the title of his essay indicates. Xenos contrasts an urban patriotism based on the political experience of the *patria* identified with one's city, on the one hand, against nationalism understood as allegiance to the abstract, large-scale nation-state, on the other, and concludes that since the concrete experience of the *patria* is no longer available to us, we should not pretend that an attachment of comparable intensity can possibly be recovered in the large nation-states of our times.

What is missing in both pictures is the tradition of republican pa-

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triotism. As I hope to be able to clarify, this interpretation of patriotism is significantly different from all the conceptions discussed by Yack and Xenos: civic and ethnic nationalism, urban patriotism, and nationalism in the sense of the allegiance to the nation-state (which Xenos equates, if I understand him correctly, with ethnic nationalism). The omission of republican patriotism may turn out to be theoretically irrelevant, but it is worth considering it, both because of the eminence of its proponents and for reasons of accuracy. I find it rather odd that in current debates on patriotism, nationalism, and cosmopolitanism, no one takes the trouble to refer to what philosophers, historians, poets, agitators, and prophets belonging to the republican family have meant to say, over the last two thousand years, when they have spoken of love of country.[1]

General Features of Republican Patriotism

For classical republican theorists, and most notably for the Romans, love of *patria* is a passion. More precisely, it is a charitable, compassionate love of the republic (*caritas reipublicae*) and of its citizens (*caritas civium*). As a Scholastic thinker later put it, "*Amor patriae in radice charitatis fundatur*": "Love for the fatherland is founded in the root of charity which puts, not the private things before those common, but the common things before the private" (Ptolemy of Lucca 1954, 299).

Even when love of country respects the principles of justice and reason, and is therefore also called a rational love (*Amor rationalis*), it is an affection for a particular republic and particular citizens who are dear to us because we share with them important things: the laws, liberty, the forum, the senate, the public squares, friends, enemies, memories of victories and memories of defeats, hopes, fears. It is a passion that grows among equal citizens, not the result of rational consent to the political principles of republics in general. Because it is a passion it translates into action, and more precisely into acts of service to the common good (*officium*) and acts of care (*cultus*).[2]

Last, it must be taken into account that for republican theorists *caritas reipublicae* is an empowering passion that impels citizens to perform the duties of citizenship and gives rulers the strength to accomplish the hard tasks necessary for the defense, or the institution, of liberty. As Livy explains in Book II, Section 2 of his *History*, where he narrates the earliest phases of the consolidation of Roman liberty

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after the expulsion of Tarquinius Superbus, it was charity toward the republic (*caritas reipublicae*) that gave Brutus the moral strength to overcome his reluctance and accomplish the unpleasant but necessary task of speaking against Lucius Tarquinius before the people of Rome. A similar use of the language of patriotism understood as *caritas* is in John Milton's *Defense of the People of England* of 1651 (VII, 455) where he writes that the execution of Charles I was an act inspired not by factiousness, or a desire to usurp the rights of others, or mere quarrelsomeness, or perverse desires, or fury or madness, but by love of country—"*patriae caritas,*" as the Latin version reads.

Examples could be multiplied, but in the context of the present discussion, I only need to make one further remark concerning the much-debated relationship between political membership in the republic and the prepolitical or nonpolitical values of the nation. For republican theorists the republic is a political ordering and a way of life; that is, a culture. To describe people's love of their republican institutions and of the way of life based upon them, Machiavelli speaks, for instance, of love of the "vivere libero." Other republicans of his time defined the republic as "a particular way of life of the city" (*una certa vita della città*; Brucioli 1982, 112).

Republican patriotism surely has a cultural dimension, but it is *primarily* a political passion based on the experience of citizenship, not on common prepolitical elements derived from being born in the same territory, belonging to the same race, speaking the same language, worshipping the same gods, having the same customs. This means that the antirepublican argument that "a purely political creed is insufficient" misses the point because republican patriotism does not rely on a purely political creed (Smith 1996, 22).

The distinction between the political and cultural values of the republic and the nonpolitical values of nationhood was rendered in Latin with the words *patria* and *natio* respectively.[3] Which of the two was considered to be more important is rather obvious. The bonds of citizenship, as Cicero put it in *De Officiis* (I.17.53), are closer and more dignified than the bonds of the *natio*. This distinction and ranking were reiterated by later theorists. Shaftesbury, for instance, complained, in his *Characteristicks of Men, Manners, Opinions, Times*, about the confusion between love of the republic and love of place and noted the inability of the English language to express the distinction clearly.

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> I must confess, I have been apt sometimes to be very angry with our language for having denied us the use of the word Patria, and afforded us no other name to express our native community than that of country, which already bore two different significations abstracted from mankind or society [*rus* and *regio*, in French *campagne* and *pays*]. Reigning words are many times of such force as to influence us considerably in our apprehension of things. Whether it be from any such cause as this, I know not, but certain it is, that in the idea of a civil state or nation we Englishmen are apt to mix somewhat more than ordinary gross and earthy. (Shaftesbury [1711] 1963, 248)

A similar warning not to confuse the *patrie* with the qualities associated with the fact of being born in a particular place was issued in even stronger terms in the *Encyclopédie* (XII, 178): there we read that *patrie* does not mean the place in which we are born, as the vulgar conception holds. It means instead the "free state" (*état libre*) of which we are members and whose laws protect our liberty and our happiness (*nos libertés et notre bonheur*). The *Encyclopédie* maintains that for Machiavelli and the republican political writers, similarly, the term *patrie* is synonymous with republic and liberty. Under the yoke of despotism there is no *patrie*, for the obvious reason that under a despotic government subjects are unprotected and excluded, precisely like strangers (ibid.) Following in Montesquieu's footsteps, the authors remarked that "those who live under the Oriental despotism, where no other law is known than the whims of the sovereign, no other maxim than the adoration of his caprices, no other principles of government than terror, where no fortune and no one are safe, those do not have a *patrie* and do not even know its name, which is the true expression of happiness" (ibid., 180).[4]

Thus, the commonplace that the Enlightenment was antipatriotic is just a gross mistake. The *philosophes* were not nationalists, but they were surely patriots in the republican sense. Equally mistaken is the widely held view that the French Revolution was inspired by, or produced, a distinctive and "powerful republican nationalism" (Smith 1996, 22). For one thing, the conception of patriotism that inspired the rhetoric of the Jacobins was precisely the classical republican interpretation of patriotism that Montesquieu, Rousseau, and the *Encyclopédie* had rephrased in the statement that love of *patrie* is love of equality.[5] Second, when the French revolutionaries spoke of the nation they meant the sovereign people, as in Sieyès's *Qu'est-ce que le*

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Tiers-Etat? They meant, more precisely, a political principle derived from natural right, with no reference whatsoever to the common culture, the history, the language, the religion, or the ethnicity of a particular people. The so-called nationalism of the French revolution was therefore completely different from nationalism proper.

I find the republican interpretation of patriotism more convincing than the contemporary view that, in order to hold heterogeneous peoples together, we need "stronger fare" than the political and cultural values of the republic, as Anthony Smith recently put it. I do not know what Smith means when he speaks of "holding together," but if he means, as I mean, to hold individuals together as free and equal citizens, we need nothing more than a politics genuinely inspired by the ideals of republican liberty and republican equality, and a culture based upon these ideals. Citizenship does not grow out of the bonds of nationhood. Peoples who are culturally, religiously, or ethnically homogeneous are not those who are the most civic minded; on the contrary, they tend to be intolerant, bigoted, and boring. Politics, a true democratic politics, can do all the work of citizenship by itself. It does not need embarrassing nationalistic helpers.

The Role of Republican Patriotism

From this outline it is rather evident, I think, that republican patriotism is different from civic nationalism, from ethnic nationalism, and from urban patriotism, as Yack and Xenos define and discuss them. Republican patriotism differs from civic nationalism in being a passion and not the result of rational consent; it is not a matter of allegiance to historically and culturally neutral universal political principles, but of attachment to the laws, the constitution, and the way of life of a particular republic. It is also distinct from ethnic nationalism because it does not attach moral or political relevance to ethnicity; on the contrary, it recognizes moral and political relevance, and beauty, in the political values of citizenship—particularly republican equality—which are hostile to ethnocentrism. And it cannot be equated with urban patriotism because it springs from attachment not to the city, but to the republic.

While the difference between republican patriotism and civic or ethnic nationalism does not need, I think, further clarification, the theme of urban patriotism does, particularly in view of Xenos's re-

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marks. The main feature of republican patriotism is not, for him, a charitable commitment to the republic and liberty, but an attachment to the city, to urban experiences. Xenos points out that *patria*, as republican political writers understood it, cannot be rendered as "nation," and therefore that patriotism cannot be equated with nationalism. He than argues that the very examples of the contemporary presence of republican patriotism that I quote in my book actually undermine my interpretation of them.

Xenos quotes and comments on the following passage from Natalia Ginzburg:

> The streets and the squares of the city, once a theater of our adolescent joy and an object of our highest scorn, became the places that we had to defend. The words "patria" and "Italy" which had made us so sick on the walls of our schools because they had been accompanied by the adjective "fascist," because they had been filled with emptiness, suddenly appeared to us without adjectives, and so transformed that it seemed to us that we heard them and thought for the first time. Suddenly they seemed real to our ears. We were there to defend the *patria* and the *patria* was those streets and those squares, our loved ones and our childhood, and all the people passing by. A truth so simple and so obvious seemed strange to us because we had grown up with the conviction that we did not have a *patria* and that we had been born, unluckily for us, at a point filled with emptiness. And even more strange was the fact that out of love for all those unknown people who were passing by, and out of love for a future we did not know but which we could make out in the distance, in the midst of deprivation and devastation, solidity and radiance, every one of us was ready to lose himself and his own life. (Ginzburg 1984, 8–9)[6]

Although Ginzburg mentions Italy in quotation marks, Xenos writes, the affection she describes

> is not toward "the nation," but rather toward the city and toward the particular streets and piazzas of individual experience. The unknown others may be unknown, but they have been seen in the public spaces of the city. That public space, that shared life, is the common good, if there is one. It is not at all abstract, and I think it is telling that when Viroli adduces an example of patriotic affection, it is framed in these urban terms. (Xenos 1996, 220)

I find Xenos's point important but not convincing. Republican patri-

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otism historically has been connected to the experience of city-republics. However, it is not based upon the attachment to the city but on love for the republic. As a distinguished member of the republican family, Jean-Jacques Rousseau, put it: "It is not the walls, nor its habitants which constitute the *patrie*, but the laws, the mores, the customs, the government, the constitution, and the way of life which results from them. The *patrie* consists of the relationship between the state and its members; when these relations change or dissolve, the *patrie* disappears" (Rousseau 1965, vol. 19, 190).[7] It is the political experience of republican liberty, or the memory, or the hope thereof, that makes a city meaningful. A city by itself is not capable of sustaining republican patriotism.

The ideal of the free republic is visible as well in Ginzburg's text. She describes an experience of discovery: the same piazzas and streets that had been meaningless to her assume a new significance, a new warmth, a new light because they have become the theater of a struggle for liberty. It is Ginzburg's new experience of commitment to political liberty, and her hope of walking on those streets and piazzas as a free citizen in the company of other free and equal citizens, which has changed her perception of those places.

For republican theorists, what makes urban places significant are common experiences and the memories of them. As Machiavelli tells us in the *Florentine Histories* (II.34), "Even if their fathers did not keep alive in the citizens the memory of liberty, the public palaces, the places of the magistrates, the ensigns of the free orders recall it." They remind the citizens of the "sweetness of the free way of life."[8] The *patria* always inspires love and attachment, but when the *patria* is joined with republican liberty, citizens love it in a special manner. People and places acquire a special beauty that stimulates feelings of closeness and sharing. The very perception of the city's beauty changes according to the significance of its places in the life and history of the republic.

Because the fundamental component of republican patriotism is love of the republic as a political and cultural institution, the problem of scale—the fact that we no longer live in city-states but in nation-states—is not so decisive, I think, as Xenos maintains. At least it was not so decisive for republicans. Mazzini was thinking of Italy, not of a city, when he wrote that "a country (*patria*) is a fellowship of free and equal men bound together in a brotherly concord of labour towards a single end. A country is not an aggregation, it is an *association*. There

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is no true country without a uniform right. There is no true country where the uniformity of that right is violated by the existence of caste, privilege and uniformity" (1972, 884).

It is perfectly true that "memories are concrete," as Xenos claims. This does not mean, however, that only memories connected to the experience of our own city are concrete. Memories of episodes that have occurred beyond the walls of our city can also be concrete, indeed can be, politically, very meaningful and moving. To see truly compassionate love of the republic and of our fellow citizens flourishing in our polities we do not need to dissolve our nations into city-states; nor do we need to reinforce linguistic, ethnic, religious, or (worst of all) moral homogeneity (if this were its price, I would rather give up patriotism). We need good government, justice, exemplary political leaders, and we need to encourage political participation. As Rousseau (1964, 258) put it: if we want the citizens to love their *patrie*, "let the homeland, therefore, show itself as the common mother of all citizens. Let the advantages they enjoy in their homeland endear it to them. Let the government leave them a large enough part of the public administration so that they can feel that they are at home. And let the laws be in their sight merely the guarantees of the common liberty."[9] In addition to that, we need to reinvent our ceremonies and to find a public rhetoric that is capable of giving warmth and color to the language of republican liberty.

Whether or not this is a desirable and feasible political project is another matter. I have argued elsewhere that it is. Here I simply want to suggest that we are not bound at all by a choice between the myth of civic nationalism, the horror of ethnic nationalism, and the dream of city-state patriotism.

NOTES

1. The best examples are the articles published in Cohen 1996.
2. I provide textual references in Viroli 1995.
3. See for instance Quintilian, *Istitutio Oratoria*, V.10.24–25.
4. "Ceux qui vivent sous le despotisme oriental, où l'on ne connoit d'autre loi que la volonté du souverain, d'autre maximes que l'adoration de ses caprices, d'autres principes de gouvernement que la terreur, où aucune fortune, aucune tête n'est en sureté; ceux là, n'ont point de *patrie*, et n'en connoissent pas même le mot, qui est la véritable expression du bonheur."
5. "Mais comme l'essence de la République ou de la démocratie est l'égalité, il

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s'ensuit que l'amour de la patrie embrasse nécessairement l'amour de l'égalité." Robespierre, "Sur les principes de morale politique qui doivent guider la convention nationale dans l'administration intérieure de la République," in Robespierre 1965, 214–15.

6. I apologize for not having given these references in the book.
7. "Ce ne sont pas les murs, ni les hommes qui font la patrie, ce sont les loix, les moeurs, les coutumes, le gouvernement, la constitution, la manière d'être qui resulte de tout cela. La patrie est dans les rélations de l'Etat à ses membres; quand ces rélations changent ou s'aneantissent, la patrie s'évanouit."
8. "E quando mai i padri non l'avessero ricordata, i palagi pubblici, i luoghi de' magistrati, l'insegne de' liberi ordini la ricordano; le quali cose conviene che sieno con grandissimo disiderio da' cittadini cognosciute. Quali opere volete voi che siano le vostre, che contrappesino alla dolcezza del vivere libero, o che faccino mancare gli uomini del disiderio delle presenti condizioni?"
9. "Que la patrie se montre donc la mere commune des citoyens, que les avantages dont ils jouissent dans leurs pays le leur rend cher, que le gouvernement leur laisse assez de part à l'administration publique pur sentir qu'il sont chez eux et que les lois ne soient a leur yeux que les garants de la commune liberté."

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Nicholas Xenos

QUESTIONING PATRIOTISM: REJOINDER TO VIROLI

ABSTRACT: *The tradition of republican patriotism articulated by Maurizio Viroli only seems to avoid the naturalizing dangers inherent in the discourse of nationalism, whether in its so-called civic or ethnic modes. Rousseau's comment that he wishes the* patrie *to be experienced as* "la mère commune des citoyens" *reflects the republican patriot's desire to find a home in the* patria. *This sentiment originated in Rome and comes down to us primarily in texts written in the immediate aftermath of the Republic's demise, a period characterized by widespread physical uprooting. The sentiment of republican patriotism can thus be seen as a nostalgic reaction to a sense of personal and political deracination and loss. In their yearning for a political home, at least, republican patriots like Rousseau share the rhetoric and desire of nationalists, and that similarity should cause us concern.*

In my article, "Civic Nationalism: Oxymoron?" (Xenos 1996), I criticized the distinction often drawn in the literature on nationalism between so-called ethnic and civic nationalisms. My point, in part, was that the rhetoric of civic nationalism betrays an underlying naturalism that is revealed in repeated allusions to the family, and that this naturalism, in turn, betrays a close affinity between the two allegedly different forms of nationalism. The persistent invocation of familial rhetoric, I suggested, is tied to the fact that the national state is an ab-

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Nicholas Xenos is professor of political science, Thompson Hall, University of Massachusetts, Box 37520, Amherst, MA 01003-7520, telephone (413) 545-6183, telefax (413) 545-3349.

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stract entity requiring an infusion of domestic imagery to sustain it emotionally. By point of contrast, I argued that the original object of patriotic discourse, the *patria*, was a more concrete entity, the city, and that the experience of the *patria* available to the Romans and to Machiavelli is available to us only fleetingly, if at all. Instead, we fall victim, often literally, to the myth-making imagery of the national family.

Maurizio Viroli's response places the question of patriotism at the center of the discussion, as he sketches out a tradition of republican patriotism in distinction from civic nationalism. His contention that I give too much credit to the city itself, rather than to "the political experience of republican liberty, or the memory, or the hope thereof" in describing the sentiment of civic affection is a point well taken. Certainly, the distinction between the city as a physical space and the city as a political entity is expressed not only by Rousseau but also in the Greek distinction between *astu* and *polis*. But to say that patriotism refers not to the city but rather to the experience of freedom in the city still leaves us with the question of how to conceptualize that experience. And in this case, Rousseau is indeed instructive, since he combined in his own experience the three possibilities Viroli mentions. Born in Geneva, he experienced the city republic in a direct way that was almost unique by the eighteenth century. Later alienated from and by it, he experienced it as a memory, which was also transformed into a hope in his *Contrat Social*, *Considérations sur le Gouvernement de Pologne*, and other writings. I therefore find it particularly interesting that Rousseau, in the comment quoted by Viroli, refers to the patrie as "*la mère commune des citoyens*" and calls upon it to furnish a sense of being "at home" to its citizens. If republican patriotism is its own source of affection and a "purely political passion," as Viroli claims, then why does an exemplary figure such as Rousseau fall back on metaphors associated with the family to express it? I want to suggest that the answer to this question lies in the experience or feeling of loss or estrangement that lies at the center of the notion of patriotism, and that in its expression, republican patriotism exhibits the same characteristics, and poses the same threats, I emphasized with regard to civic nationalism.

Viroli is right to take us back to Rome in order to understand the origins of the idea of patriotism, but on further reflection I find those origins to be complex and not easily comprehended by a modern sensibility. For the Romans, a walled city was an *urbs*, and the word

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alone could mean Rome in the same sense meant by people who live in the vicinity of New York City when they say simply, "I'm going to the city." But whereas *polis* to the Greeks referred obviously to a particular political entity, *patria* is harder for us to grasp. It is not a synonym for *polis*, but "Rome" is clearly signified by a Roman's reference to his *patria*, though any individual "Roman" could have been born in another city. Indeed, Cicero addresses this situation in a section of his dialogue on the laws, *De Legibus*, when he suggests that everyone has not one but two *patriae*. The dialogue takes place at Cicero's country estate in Arpinium, and Cicero remarks that he has two *patriae*: Arpinium, the place where he was born and where his family ancestors are buried, and Rome, the place where he receives his citizenship (*De Leg.* II. 5.) In making this argument, Cicero relies upon what is in essence a familial metaphor to describe the *patria* of his citizenship. The *patria* in which we find our political identity is like the *patria* of our *genus,* in which we find our ancestral identity, but they are neither equivalent nor the same.[1] Cicero wants to explain the nature of his emotional attachment to Rome, and this attachment is in part the political passion of which Viroli speaks, but to convey it he has to liken it to a more domestic passion. The ancestors and religious rites and sites of his familial identity are paralleled by the great men of the past, the public gods, and the public buildings of Rome. It is a short step, I think, to a confusion of the two realms and toward the sort of sacrificial demands made of the polity in the name of the imagined political family that I find manifested in Lincoln's Gettysburg rhetoric, for example (Xenos 1996, 224–28).

To clarify my point, I think it is important to note that it is only at the end of the republic that this conception of republican patriotism is articulated by Cicero. Viroli cites the example given by Livy of Brutus after the expulsion of Tarquinus Superbus to emphasize the enabling power of love of the *patria*. This example involves Brutus's reluctant endorsement of his friend Lucius Tarquinus's expulsion from Rome. Even more compelling is the virtue displayed by Brutus in presiding over the execution of his sons for conspiring against the new Republic, for this example shows the need for love of the second *patria* to take precedence over love of the first. The *patria potestas*, the power of the father over the lives of his children, was well established in the Roman tradition, but it was a power almost always held in abeyance. In the story of Brutus, this power is exercised by metaphor, we might say, since he was both the actual father of these

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young men and the founding father of the Republic. All eyes were on Brutus's face as his sons were executed, Livy tells us, because this act was thought to be so horrifying (II v. 5–9). His face registers both the anguish of the father and the steadfastness of the political *pater* (and we are reminded that the Roman Senators were later called the *patres*, or fathers).

But it is also important to remember that these examples are given us long after the fact, and indeed after the demise of the Republic itself, since Livy completed the early books of his History around 27–25 BC. Cicero was a victim of the civil wars that ravaged the late Republic, while Livy, who was born and died at Patavium, the present-day Padua, wrote during what came to be known as the Augustan Age. The Romans were acutely conscious of the fact that they, unlike the Athenians (or so they believed), were not autochthonous. It was in the Augustan Age that the multiple stories of Rome's origins that had coexisted without concern for consistency suddenly were supplanted by the narrative power of great literary works. Livy's *History*, together with Virgil's *Aeneid*, established a definitive version of Rome's origin and subsequent history, one that helped to legitimate the new regime of Augustus, in which he was declared *pater patriae*, "father of the *patria*" (Gruen 1992, ch. 1). These works had the effect of narrating a genealogy of the political family of Rome.

It is perhaps not too much to speculate that the emphasis upon love of the *patria* in such a period is tied to the upheavals that characterized it, both politically and domestically. Not only had the Republic been lost, but many Romans had suffered through the land expropriations by which Octavian had rewarded his soldiers and punished his enemies (this was likely true for both Virgil and Horace). Cicero, a member of the equestrian class, could walk among the traces of his familial ancestry at Arpinium, but thousands of less fortunate Romans were newly provided with land and had to look instead to Rome itself for their genealogy. Their love of the *patria* might be unrivaled. They might take pride in the history of the Republic, but that history was now enveloped in the Empire. I suspect that it was then that some indistinct threshold of abstraction was crossed.

The French philosopher Simone Weil (1996, 45) wrote, in the middle of our millennium, that "our patriotism comes straight from the Romans," and she meant by this that patriotism is a kind of idolatry indulged in by the uprooted and that in modernity we are all uprooted. This is as true of the itinerant Rousseau as it is for seemingly

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settled citizens of liberal democracies today.[2] Behind the republican patriotism of Rousseau I hear a yearning to return home that is echoed in the language of civic nationalism today. And it still makes me nervous.

NOTES

1. I have interpreted this passage in detail in an unpublished paper entitled "A Patria to Die For," which was presented at the 1997 Annual Meeting of the International Studies Association.
2. Machiavelli, who had an immediate experience of his city republic, is notable in this regard since he never, I believe, uses familial metaphors to describe his *patria* or that of the Romans. If anything, this increases the distance between ourselves and Machiavelli that I emphasized in my article, and it suggests that a peculiarly medieval experience of urban cosmopolitanism was still resonant in Machiavelli. This may well signal a rupture in history of civic republicanism, rather than a tradition stretching forward from the Romans, as described by Viroli.

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Bernard Yack

CAN PATRIOTISM SAVE US FROM NATIONALISM? REJOINDER TO VIROLI

ABSTRACT: *Viroli is right to draw a distinction between republican patriotism and nationalism. But in arguing that the former can correct the problems associated with the latter, he places too much trust in the descriptions of patriotism offered by republican theorists. In practice, republican patriotism has been almost as fierce and hostile to outsiders as nationalism. Patriotism might make us better citizens, but it will not make the world a more peaceful or generous place.*

What should we make of the difference between patriotism and nationalism? Given the way in which they are talked about today, it is tempting to describe them as little more than positive and negative terms for the same phenomenon and to conclude, paraphrasing Hobbes, that nationalism is patriotism misliked and patriotism, nationalism liked.

But Professor Viroli has given us good reason to resist this temptation. In his fine book *For Love of Country* (Viroli 1995), as well as in the current issue of *Critical Review* (Viroli 1998), he guides us through the long tradition of political discourse about patriotism and highlights the important ways in which it departs from discourse about

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Bernard Yack, Department of Political Science, University of Wisconsin-Madison, Madison, WI 53706, telephone (608) 263-2414, is the author of *The Longing for Total Revolution* (Princeton, 1986) and *The Problems of a Political Animal* (University of California, 1993), and the editor of *Liberalism without Illusions* (University of Chicago, 1996).

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nationalism. Patriotism, in this tradition, is "love of the political institutions and the way of life that sustain the common liberty of a people" (Viroli 1995, 1). It differs from ethnic nationalism in its focus on the political values of citizenship. And it differs from civic nationalism—which Viroli agrees with me in characterizing as a mixture of self-congratulation and wishful thinking on the part of some contemporary liberals—in its enthusiastic embrace of a collective passion, the love of the republican patria.

Why then, Viroli wonders, have the participants in today's heated debates about nationalism and cosmopolitanism paid so little attention to a way of talking about shared sentiment and identity that promises a combination of local loyalties and principled devotion to the cause of liberty? Lack of familiarity with republican patriotism and failure to distinguish it from nationalism seem to be the culprits for him, shortcomings that he seeks to correct with careful historical scholarship.

I would like to suggest, however, a different answer to Professor Viroli's question. Contemporary intellectuals have been reluctant to invoke the language of republican patriotism as an "antidote to nationalism" (Viroli 1995, 8-9), I believe, because the practice of republican patriotism has never lived up to the shining image fashioned by its leading advocates. Dressed up in the language of its most eloquent partisans, from Cicero to Shaftesbury to Robespierre, republican patriotism looks like a generous love of one's compatriots that "sustains liberty instead of fomenting exclusion or aggression" (ibid.) against outsiders. Republican love of country, says Benedetto Croce in a passage Viroli quotes with approval, is as different from nationalism as a "gentle love for another human being" differs from "bestial lust, diseased luxury, and selfish whim" (ibid., 168). In short, republican patriotism provides us, in theory at least, with the solidarity of particularism without its nasty side-effects, for "the love of common liberty *easily* extends beyond national borders and is translated into solidarity" (ibid., 12, emphasis added).

Unfortunately, this antidote to nationalism is merely a form of discourse, i.e., a way of *talking* about our shared sentiments and identities. As Viroli ably demonstrates, there is a long tradition of discourse characterizing republican patriotism as a generous passion that inspires love for fellow citizens without inspiring hostility to outsiders. But how accurate is that characterization? Does republican patriotism in practice live up to this vision of a gentle and generous sentiment?

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Even a quick look at the historical record suggests that it does not. Republican patriots may have inspired many memorable and useful virtues over the centuries, but gentleness and sympathy toward outsiders are not prominent among them.

Cicero, the founder of the republican tradition that Viroli studies, may characterize patriotism as a form of compassion and respect. But does the history of the Roman republic give us much reason to think of patriotism as a love that "sustains liberty instead of fomenting exclusion or aggression"? I think not, unless you are willing to believe that the Roman republic conquered the Western world in response to a never-ending series of unjust and unprovoked attacks by its neighbors. Shaftesbury may urge us to follow the gentle and generous example of ancient Greek patriots who "were yet so far from a conceited, selfish, and ridiculous contempt of others, that they were even, in a contrary extreme, admirers of whatever was in the least degree ingenious or curious in foreign nations" (cited in Viroli 1995, 59). But he ignores the extraordinary ferocity of the Greeks' never-ending wars with neighboring cities and peoples. The patriots of the French Revolution may describe their love of country as a generous sentiment that they wished to share with the world. But when the world resisted their embrace, they quickly learned to march to the chorus of the Marseillaise as it urged French patriots to "let impure blood water our fields." Wherever republican patriotism has risen to the level of a shared passion—and it is a shared *passion* that Viroli is calling for—it has been a fierce rather than a gentle passion that patriots have displayed.

The problem with nationalism is not its lack of a positive vision of national loyalties, a vision that connects love of one's own to respect and admiration for others. Like republican patriotism, nationalism has had many partisans who describe it in this way. Herder, for example, urges us to cherish each nation as a unique facet of humanity, just as republican patriots teach us to cherish the liberty of each community. The problem in each case lies in the actual sentiments that their followers express, not in the way they describe these sentiments. Republican patriotism does not look like an antidote to nationalism because it seems, in practice, to promote the same kind of irritable pride and hostility toward others that we associate with nationalism. A strong dose of it might make us juster and more conscientious citizens, but I doubt that it would make the world a more peaceful or generous place.

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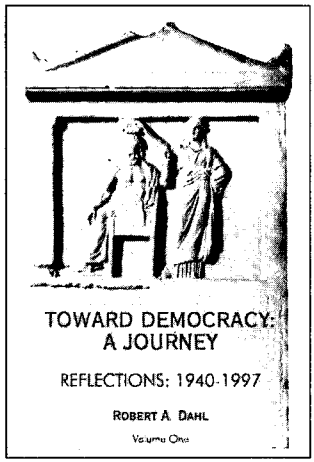

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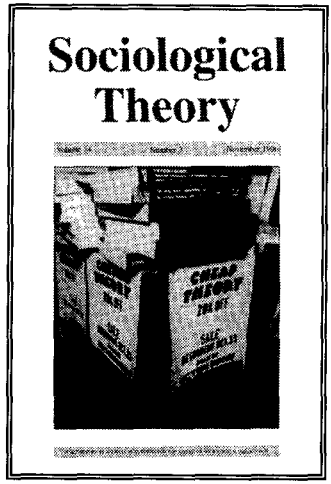

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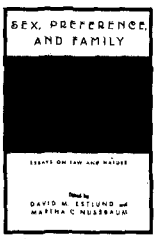

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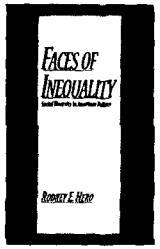

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